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THE
BRITISH JOURNAL
OF
MEDICAL PSYCHOLOGY

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BEING THE MEDICAL SECTION OF THE
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Edited by
T. F. MAIN AND JOSEPH SANDLER

with the assistance of

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VOLUME XXXII, 1959



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*Printed in Great Britain at the University Press, Cambridge
(Brooke Crutchley, University Printer)
and published by the Cambridge University Press
Bentley House, 200 Euston Road, London, N.W.1
American Branch: 32 East 57th Street, New York 22, N.Y.
Agent in Canada: The Macmillan Company of Canada Limited*

Bureau Edn.	Research
Dated	27. 8. 70
Access No.	J 232

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THE EFFORT TO DRIVE THE OTHER PERSON CRAZY—AN ELEMENT IN THE AETIOLOGY AND PSYCHOTHERAPY OF SCHIZOPHRENIA*

By HAROLD F. SEARLES

Among all the factors in the aetiology of schizophrenia, factors which are undoubtedly complex and, further, considerably variable from one case to another, there appears to be one specific ingredient which can often—and even, I believe, regularly—be found to be operative. My clinical experience has indicated that the individual becomes schizophrenic partly by reason of a long-continued effort, a largely or wholly unconscious effort, on the part of some person or persons highly important in his upbringing, to drive him crazy.

I well know that it would be inane to reduce the complex aetiology of schizophrenia to a simple formula stating that an individual becomes schizophrenic because some other individual drives him crazy. Such a formula would not do justice to the individual's own psychological activity in the situation, to the complexity of that particular interpersonal relationship, to the complex group-processes of the family situation, or to the larger socio-dynamic processes in which the family plays but a part—often a part in which the family as a whole is helpless to deal with large and tragic circumstances quite beyond any family's capacity to control or avert.

Previous literature

The only writings about this subject which I have found in the professional literature are statements by Arieti (1955), and by that group of researchers at the Mayo Foundation which is headed by Johnson (Beckett *et al.*, 1956; and Johnson, Giffin, Watson & Beckett, 1956),

and these statements have done little more than touch upon the subject, without exploring it in detail.

Arieti describes what he terms 'acted-out' or 'externalized' psychoses, explaining that: '... These persons often create situations which will precipitate or engender psychoses in other people, whereas they themselves remain immune from overt symptoms.'

Johnson and her co-workers, reporting upon the concomitant psychotherapy of schizophrenic patients and the members of these patients' families, emphasize that this experience confirmed the authors' initial impression that '... in some cases parental expression of hostility through a child might both determine psychosis in the child and protect the parent from psychosis' (Beckett *et al.* 1956). In many instances they found a history of psychological assault by the parent(s) upon the child, assault of a type which was specifically reflected in the patient's earliest delusions. It is of special interest here that among the various types of assault they describe were '... threats that insanity may develop in the patient'.

Hill (1955), while nowhere formulating the particular concept which I am describing in this paper, presents a picture of a symbiotic patient-parent relationship which constitutes a conceptual background into which my concept fits, I believe, precisely. He says that the mother (or, in occasional instances, the father): '... Makes the conditions for [the child's] security in living those which meet her own defensive and aggressive requirements to avoid psychosis.' '... One meaning of the futility of the dependence-independence struggle of the schizophrenic... is his belief, based upon his

* Revised Manuscript received 19 September 1958.

observations, that, if he should improve and become well in the normal sense, his mother would become psychotic....'

Bowen (1956), as a result of concomitant psychotherapy of schizophrenic patients and their families, has reached conclusions similar to those of Hill. Reichard & Tillman (1950), Lidz & Lidz (1952) and Limentani (1956) are among the other writers whose discussions of symbiotic relatedness are relevant to my paper.

My theoretical formulations will be presented, with such brief samples of clinical documentation as space will allow, in the following categories: (A) *modes* of one's driving the other person crazy, (B) *motives* behind the effort to drive the other person crazy, and (C) this mode of interaction in the *patient-therapist relationship*.

A. MODES OF DRIVING THE OTHER PERSON CRAZY

In trying to delineate the modes or techniques which are employed in one person's effort to drive another person crazy—or, in our professional terminology, schizophrenic—I cannot overemphasize my conviction that the striving goes on at a predominantly *unconscious* level, and my conviction that this is but one ingredient in a complex pathogenic relatedness which is well beyond the capacity of either one, or both, of the participants to control fully.

In general one can say, I think, that *the initiating of any kind of interpersonal interaction which tends to foster emotional conflict in the other person—which tends to activate various areas of his personality in opposition to one another—tends to drive him crazy (i.e. schizophrenic)*.

For example, a man in analysis is reported by his wife to be persistently 'questioning the adjustment' of her younger sister, an insecure young woman, until the girl becomes increasingly anxious; he does this, evidently, by repeatedly calling her attention to areas of her personality of which she is at best dimly aware, areas which are quite at variance with the person she considers herself to be. The re-

pressions which have been necessary for the maintenance of a functioning ego are thereby weakened (without actual psychotherapy being available to her), and increasing conflict and anxiety supervene. Quite similarly, it can be seen that the inexperienced or unconsciously sadistic analyst who makes many premature interpretations is thereby tending to drive the patient psychotic—tending to weaken the patient's ego rather than, in line with his conscious aim, to strengthen that ego by helping the patient to gradually assimilate previously repressed material through more timely interpretations.

Or a person may stimulate the other person sexually, in a setting where it would be disastrous for that person to seek gratification for his or her aroused sexual needs; thus, again, a conflict is produced. We see this in innumerable instances from schizophrenic patients' histories, in which a parent behaved in an inordinately seductive way toward the child, thus fostering in the latter an intense conflict between sexual needs on the one hand, and rigorous super-ego retaliations (in line with the taboo of the culture against incest) on the other hand. This circumstance can also be seen as productive of a conflict in the child between, on the one hand, his desire to mature and fulfil his own individuality, and on the other hand his regressive desire to remain in an infantile symbiosis with the parent, to remain there at the cost of investing even his sexual strivings—which constitute his trump card in the game for self-realization—in that regressive relationship.

The simultaneous, or rapidly alternating, stimulation-and-frustration of other needs in addition to sexual ones can have, I believe, a similarly disintegrating effect. One male patient, emerging from a psychosis in which his intense ambivalent feelings toward his mother played a central part, became able to describe something of his childhood-relationship with her. The mother's rejecting attitude was high-lighted by his recollection that he had never seen her kiss his father, whom the mother had dominated and nagged mercilessly.

The patient remembered that there was one occasion when the mother had *started* to kiss her husband. This was at a time when, late in the son's childhood, his father was being rolled into the operating room of a hospital, for a major operation following a car accident. The mother leaned down as though to kiss her husband and the patient saw his face become suffused with joyous anticipation. Then the mother thought better of it, and straightened up. The patient described this with a desolate feeling-tone, as though he himself had experienced this sort of frustration at her hands many times in his own life.

Similarly, with the child's desire, as well as felt duty, to be helpful to (for example) a parent: frequently we find in the histories of schizophrenic patients that one or both the parents made chronic pleas for sympathy, understanding, and what we would call in essence therapeutic intervention, from the child, while simultaneously rejecting his efforts to be helpful, so that his genuine sympathy and desire to be helpful became compounded with guilt, rage, and, perhaps above all, a sense of personal helplessness and worthlessness. In this connexion, Bateson, Jackson, Haley & Weakland (1956) have described parental injunctions of a mutually contradictory or 'double bind' nature as being important in the aetiology of schizophrenia.

Another technique, closely related to the stimulation-frustration technique just described, is that of one's dealing with the other person upon two (or perhaps even more) quite unrelated levels of relatedness simultaneously. This tends to require the other person to dissociate his participation in one or another (or possibly both) these levels, because he feels it to be so crazily inappropriate that he should find himself responding in terms of that particular level, since it seems to be utterly unrelated to what is going on at the other, more conscious and overt, level.

For example, on one or two occasions in my years-long work with a physically attractive

and often very seductive paranoid schizophrenic woman, I have felt hard-put to keep from going crazy when she was simultaneously (a) engaging me in some politico-philosophical debate (in which she was expressing herself with a virile kind of forceful, businesslike vigour, while I, though not being given a chance to say much, felt quite strongly urged to argue some of these points with her, and did so); and (b) strolling about the room or posing herself on her bed, in an extremely short-skirted dancing costume, in a sexually inflaming way. She made no verbal references to sex, except for charging me, early in the hour, with having 'lustful', 'erotic' desires; from there on, all the verbal interaction was this debate about theology, philosophy, and international politics, and it seemed to me that the non-verbal interaction was blatantly sexual. But—and here is, I think, the crucial point—I felt no consensual validation (at a conscious level) from her about this more covert interaction; this non-verbal sexual interaction tended to appear as simply a 'crazy' product of my own imagination. Even though I knew there was a reality basis for my responding on these two unrelated levels, I still found it such a strain that I felt, as I say, as though I were losing my mind. An insecure child, engaged in such a broadly divided interrelatedness with a parent would, I think, suffer significant personality trauma in an oft-repeated situation of this sort.

Another technique, closely akin to that of relating to the other person upon two or more disjointed levels at once, is the sudden switching from one emotional wavelength to another, such as one finds so very frequently among the parents of schizophrenic patients. For example, one deeply schizophrenic young man's mother, a very intense person who talked with machine-gun rapidity, poured out to me in an uninterrupted rush of words the following sentences, which were so full of *non sequiturs*, as regards emotional tone, that they left me momentarily quite dazed: 'He was very happy. I can't imagine this thing coming over him. He never was down, ever.

He loved his radio repair work at Mr Mitchell's shop in Lewiston. Mr Mitchell is a very perfectionistic person. I don't think any of the men at his shop before Edward lasted more than a few months. But Edward got along with him beautifully. He used to come home and say [the mother imitates an exhausted sigh], "I can't stand it another minute!" The patient, for several months prior to his hospitalization, had spent most of his time at home in the company of his mother, and I thought it significant, in this same connexion, that during the early months of his hospital stay he showed every evidence (in his facial expressions and so on) of being assailed by upsurging feelings which changed in quality with overwhelming suddenness and frequency. For example, one moment his face would show a mixture of hatred and loathing, then he would suddenly jerk as though struck by some massive object, while his face showed now an intense grief.

My implication, that this phenomenon was partly a result of his long-continued exposure to his mother's poorly integrated personality, is not intended to rule out the possibility that the process worked in the reverse direction at the same time. On the contrary, I was impressed with the mother's evidencing a better integration after the patient had been out of the home for some time, and thought it entirely likely that during my above-mentioned interview with her, at the time of her son's admission, she was showing some of the after-effects of years-long exposure to an extremely poorly integrated, psychotic person, with whose capacity to assault one's own integration I myself became most uncomfortably acquainted in the course of my work with him. All this touches upon the matter of struggle, between child and parent or between patient and therapist, to drive one another crazy; I shall go into this matter later on.

The now-deceased mother of another schizophrenic man was described by the patient's siblings as having been completely unpredictable in her emotional changeability; for instance, she would return from the synagogue with a beatific expression on her face, as though she were immersed in some joyous

spiritual experience, and two minutes later would be throwing a kitchen-pot at one of the children. At times she was warm and tender to the patient, but would suddenly lash out at the child with virulent accusations or severe physical beatings. The patient, who at the time of my beginning therapy with him had been suffering from paranoid schizophrenia for some years, required more than three years of intensive psychotherapy to become free of the delusion that he had had not one mother but many different ones. He would object repeatedly to my reference to 'your mother', protesting that he never had *one* mother; once he explained, seriously and utterly convincingly: 'When you use the word 'mother', I see a picture of a parade of women, each one representing a different point of view.'

A continual, unexpected switching from one conversational topic to another without, necessarily, any marked shift in feeling-content is in itself a mode of interpersonal participation which can have a significantly disintegrating effect upon the other person's psychological functioning, as can be attested by any therapist who has worked with a patient who shows prolonged and severe confusion.

Each of these techniques tends to undermine the other person's confidence in the reliability of his own emotional reactions and of his own perception of outer reality (a formulation for which I am indebted to Dr Donald L. Burnham). In one of the previously mentioned papers by Johnson *et al.* (1956) we find the following pertinent description of the schizophrenic patients' childhood relationships with their parents: '...When these children perceived the anger and hostility of a parent, as they did on many occasions, immediately the parent would deny that he was angry and would insist that the child deny it too, so that the child was faced with the dilemma of whether to believe the parent or his own senses. If he believed his senses, he maintained a firm grasp on reality; if he believed the parent, he maintained the needed relationship, but

distorted his perception of reality. Repeated parental denial resulted in the child's failure to develop adequate reality testing.*

The subject about which I am writing in this paper ties in with one from a quite different field of human activity: international politics and warfare. I refer to the subject of brainwashing and allied techniques. In reading a recent and valuable book upon this subject by Meerloo, entitled, *The Rape of the Mind* (1956), I was repeatedly impressed with the many similarities between the conscious and deliberate techniques of brainwashing which he describes, and the unconscious (or predominantly unconscious) techniques of thwarting of ego-development, and undermining of ego-functioning, which I have found to be at work in the experiences, both current and past, of schizophrenic patients. The enforced isolation in which the brainwashee exists—isolated from all save his inquisitor(s)—is but one example of these similarities; in the life of the to-be-schizophrenic child, a regular accompaniment of the parent's integration-eroding kinds of behaviour is an injunction against the child's turning to other persons who might validate his own emotional reactions and assure him against the parent-inspired fears that he must be 'crazy' to have such 'irrational' reactions to the parent.

Meerloo's book describes brainwashing and allied techniques as occurring in the form of (a) deliberate experiments in the service of totalitarian political ideologies; and (b) cultural undercurrents in our present-day society, even in politically democratic countries. My paper portrays much these same techniques as occurring in a third area: the lives of schizophrenic patients.

* One of my patients, who throughout his childhood was told, 'You're crazy!' whenever he saw through his parents' defensive denial, became so mistrustful of his own emotional responses that he relied heavily, for years, upon a pet dog to let him know, by its reaction to this or that other person whom he and his pet encountered, whether the person were friendly and trustworthy, or hostile and to be on guard against.

B. MOTIVES BEHIND THE EFFORT TO DRIVE THE OTHER PERSON CRAZY

A mode of interpersonal participation which tends to drive the other person crazy can be based, seemingly, upon any one of a wide variety of motives; in any single instance, probably a complex constellation of various motives are at work. These motives range all the way, apparently, from intense hostility on one end of the scale to, at the other end, desires for a healthier, closer relatedness with the other person, and desires for self-realization. I shall start with those more obvious motives at the former end of the scale.

(1) The effort to drive the other person crazy can consist, predominantly, in the psychological equivalent of murder; that is, it can represent primarily an endeavour to destroy the other person, to get rid of him as completely as if he were physically destroyed. In this connexion, it is interesting to note that whereas our legal system reserves its severest punishment for him who commits physical murder, it metes out no—or at most negligible—punishment for psychological 'murder', for destroying another person psychologically by driving him 'crazy'. In the knowledge of the average person who is unacquainted with the details of legal procedure, the only legal penalty in this area is the tangentially relevant and entirely unfrightening legal charge of 'mental cruelty' which, he knows, is not infrequently conjured up as an excuse to make a high percentage of divorces obtainable.

I do not mean to imply that I wonder at this legal state of things, or that I suggest any change in the law in this regard; I think it would be impracticable to set out to prove, legally, that one person had contributed significantly to another person's 'going crazy'. My point is that this state of things does exist in our legal system, such that whereas one has a reason to feel deterred, by law, from physical murder, one has practically no reason to feel similarly deterred from what might be thought of as psychological murder.

It should be noted, further, that a psychosis

which is severe enough to require years-long hospitalization does indeed serve to bar the patient from continued participation in the life of, for instance, a family, almost as effectively as would death itself. It is not unheard of for the parents of a long-psychotic, hospitalized child to let it be known that the child has died, and it is much more frequent that the family members still at home avoid making references to the patient, in their everyday life with friends and associates, and avoid consulting or informing the patient concerning family crises, very much as though the patient had 'passed away'.

As an example of this kind of motive, I shall cite certain data from my work with a young woman who had been hospitalized for more than three years for a schizophrenic illness, and who by this time had become able to tell me some details about her life in her family prior to the onset of her illness.

She had one sibling, a sister two years younger than herself. Both girls were good looking; both had been strongly indoctrinated with the view, from their mother and father, that a girl's only *raison d'être* is the acquisition of a socially prominent and wealthy husband; both were much involved in fantasies of being the wife of the father, since their mother accepted a much-derogated role in the family. They were, therefore, intensely and openly competitive with one another.

My patient, in one of her psychotherapeutic sessions, reminisced about a time (not more than two years prior to her first hospitalization) when her sister had been jilted by a boy friend whom the sister had introduced to a supposed friend, named Mary. She said that for about a year after that, her sister wore dark glasses and 'went around the house talking about suicide', and weeping. The patient said that the glasses 'were driving her [i.e. the sister] crazy'. She also added, 'My sister used to say she read a lot so that she wouldn't go nuts', and commented to me that 'the jealousy and hatred . . . and all the teasing . . . make a person wild.' She spoke of 'how jealous Sarah [the sister] was of Mary', giving me to think that she was about to say 'of me', but shifted to

'of Mary'; I got the distinct impression, from other things she said, that the jealousy between herself and her sister was intense during that period. I noticed that when she spoke, from time to time, of the suffering her sister had evidenced, a sadistic smile repeatedly came over her face. She said at one point that 'If two people each want the same thing' they're bound to have hatred and jealousy toward one another, and later spoke of how much hatred and jealousy one has toward somebody who is standing in the way of something or somebody one wants. I commented, here, casually, that naturally one feels like killing the other person, getting rid of them. She replied, 'Killing—that isn't allowed—', as if she had already considered that but had come up against the fact that, for some reason incomprehensible to her, this was forbidden.

Parenthetically, this girl's case history describes her having verbally threatened to murder her sister—'I'll get you in the back when you're not looking'—and having picked up a hammer and threatened to kill her mother with it. The sister, who had been married a few months after the patient's initial hospitalization, was afraid to let the patient visit her for fear the latter would kill the sister's small baby; in short, the family took her threats of murder quite seriously.

Now, when she said thoughtfully, during this therapeutic session, 'Killing—that isn't allowed—', she added significantly, '—but there are other ways'. On another occasion, while telling of her sister's depressive symptoms, she fell to reciting the words of the nonsense song 'Mairzy Doats', which had been popular during that era. She puzzled, in her chronically confused way, over the word 'Mairzy', saying twice that sometimes the word is 'Mairzy' and sometimes it is 'Mary', giving me every impression that during the sister's depression she had tormented the sister by asking her about this song, often using the hated word 'Mary', the name of the sister's former friend who had taken from her, and eventually married, her steady boy friend.

This material is too long to reproduce in

full here; but, in essence, it should be reported that the patient had evidently felt herself to be in a desperate struggle with the sister as to which one would drive the other crazy first. On one occasion the patient recalled, with obvious anxiety, 'Sarah said I had something to do with it', the 'it' referring to Sarah's depression, and she quoted Sarah as saying, 'I hope you never get this', which evidently had an ominously threatening connotation to the patient. I got every impression that she felt guilty about the sister's illness, felt that the sister blamed her for it, and feared that the sister would vengefully cause her to be similarly ill, in retaliation.

This touches upon the subject of what I call 'psychosis wishes', entirely analogous to 'death wishes'. On several occasions, when working with patients who have had an experience, earlier in life prior to their own illness, of a parent's being hospitalized because of a psychotic illness, I have found that the patients show guilt about repressed 'psychosis wishes', entirely similar to 'death wishes' which are productive of guilt in persons who have lost a hated parent through death. The patients who show this guilt over 'psychosis wishes' show every evidence of feeling that they were once successful in a mutual struggle with the parent, in which each was striving to drive the other crazy, and the subsequent appearance of their own psychosis seems to be attributable in part to guilt, and fear of the parent's revenge, stemming from that duel in previous years. In the case which I have been describing, the sister was not actually hospitalized; but in other respects the circumstances were those which I have just outlined as regards patients whose mother or father, in their childhood, had been hospitalized with psychosis.

In this girl's family life, the particular mode of interpersonal participation with which I am dealing in this paper—the effort to drive the other person crazy—seems to have been a customary mode, over the years, of the various family members' interaction with one another. I shall cite but one more portion of the

available data. During childhood and adolescence, she had experienced a great deal of anxiety about her teeth, partly on account of her having lost some of them in a playground accident. Her father used to frighten her, time and again, by telling her teasingly, 'I'm going to take your teeth out and use them for golf tees.' For the first several months after her admission to Chestnut Lodge she was, in the words of the psychiatric administrator here, 'crawling with terror', incessantly demanding reassurance that no harm would come to her teeth and to various other body-parts. After several years of therapy she made clear to me her conviction that her family members, each of them possessing much hatred and envy of her, had acted in concert to drive her crazy and thus rid their home of her presence, and although this is by no means an accurate total picture of what had happened, it is, I believe, an accurate description of a part of what had happened.

(2) The effort to drive the other person crazy can be motivated predominantly by a desire to externalize, and thus get rid of, the threatening craziness in oneself. It is well known that the families of schizophrenic patients have a proclivity for dealing with the patient as being 'the crazy one' in the family, the repository of all the craziness among the various other family members. Hill's previously mentioned book (1955) contains some valuable observations which help one to grasp the concept that the patient's craziness consists, to a significant degree, in an introjected crazy parent (usually, in Hill's experience as in my own, the mother), a parent who now, introjected, comprises the predominance of the patient's own irrational and cripplingly powerful superego. To the extent to which this process takes place in the mother's relationship with her child, she succeeds, in effect, in externalizing upon him her own 'craziness'. My concept of one's striving to drive the other person crazy emerges naturally, as I mentioned earlier, from many of the interesting formulations at which Hill has arrived.

I have presented elsewhere (1958) my view

that a most important ingredient in the above-described mother-child relatedness is the child's genuine love and solicitude for the mother, love and solicitude of such a degree as to impel him to collaborate with her in this pathological integration. He loves her so deeply, in short, that he sacrifices his own developing individuality to the symbiosis which is so necessary to her personality-functioning.

(3) Another motive behind this effort which I am describing is, in many instances, the wish to find surcease from an intolerably conflictual and suspenseful situation. If one's mother, for example, recurrently holds before one the threat that she will go crazy, with the implication that it will be catastrophic for oneself if this indispensable person were thus to remove herself from the situation, one may well be tempted to do one's utmost to *drive* her crazy and thus to cut, oneself, the thread that holds this sword of Damocles over one's head; if it is so apt to fall in any case, one can at least salvage the satisfaction of feeling that it is one's own hand which has effected the unavoidable catastrophe.

We see, every day in our work in psychiatry, that patients tend to bring upon themselves any catastrophe which is sensed as being inevitable, in their effort to diminish intolerable feelings of helplessness and suspense in the face of it.

The tormentingly insecure nature of the ever-ambivalent symbiotic relatedness which existed between the schizophrenic, in infancy and childhood, and his mother or father, has been described by Hill (1955) Arieti (1955), Bowen (1956) and myself (1951). Any one who has participated in a long-continued therapeutic endeavour with a schizophrenic patient has experienced at first hand, re-enacted in the transference relationship between the patient and himself, the intensely ambivalent—*mutually* so—relatedness which existed between the patient and the pathogenically-more-significant parent.

(4) With surprising frequency, one finds, both in patients' histories and, much more impressively, in the unfolding of their child-

hood relationships with the parents in the evolution of transference phenomena, that they had come to the discovery, over the course of the years of their childhood, that one or another of the parents was, so to speak, a little crazy. They felt—often rightly, I think—that the evidence of the parent's craziness was so subtle, or so hidden from public display and released only in their own relationship with the parent, that only the child himself was aware of the full extent of it. In these circumstances, this knowledge remains as a guilt-laden secret in the child; he strongly tends to feel somehow responsible for the fact of the parent's craziness, and heavily burdened by both the craziness—since the parent seeks satisfaction for the psychotically expressed needs from this child in particular—and by his own knowledge of its existence.

Thus the setting is ripe for his becoming tempted to foster the parent's becoming sufficiently openly psychotic—tempted, that is, to drive the parent into craziness which will be evident to others beside himself—so that the family and the larger community will share his own burden. The patients one encounters who have had this kind of pre-psychotic experience are much more numerous than those who have had the experience of one or another parent's becoming openly psychotic and requiring hospitalization.

In my presentation of this formulation, I do not lose sight of the likelihood that a patient who is himself struggling against a developing psychosis will project his own threatening 'craziness' on to one or another parent. This happens often and even, I believe, regularly. But the process which I have described occurs, not infrequently, in addition.

(5) One of the most powerful and frequently encountered of the motives behind the effort in question is a desire to find a soul-mate to assuage unbearable loneliness. In the case of every one of the schizophrenic patients with whom I have worked long and successfully enough to perceive the childhood relationships relatively clearly, this motive evidently had been at work in whichever parent had inte-

grated a symbiotic relatedness with the child. The precariously integrated parent is typically a very lonely person who hungers for someone to share her (or his) private emotional experiences and distorted views of the world.

The following report by an attendant, concerning his conversation with a 28-year-old male schizophrenic patient, shows this motive at work:

Carl had been quiet—seemingly depressed all morning—when he suddenly started to talk about his mother's illness. Said he envied his older sister because she didn't have to bear the brunt of his mother's illness. (The sister is not sick.) Said that his mother 'tried out' her paranoid ideas on him. Would go around the house, pull down the blinds, check to see if any one were near, then tell him what apparently were full blown paranoid ideas about the neighbours and friends. [He] very philosophically announced that he felt she needed company in her illness—that she felt so lonely that she had to use him in this way... [He] has ideas people are talking about him, and voices these ideas in a naïve, forthright manner.

This parental motive is reflected in patients' fanatical loyalty to the parent, a loyalty which gives way, in the psychotherapy of chronic schizophrenia, only after years of arduous work by patient and therapist. One finds evidence of it, too, in the frequency with which deeply ill patients hallucinate this parent in an idealized form, an idealized parent often, from what I have discerned in my own work, split into two, one the personification of evil and the other the personification of loving protectiveness. With the most deeply ill patients it may require many months of stressful therapeutic labour before the therapist begins to assume, in the patient's view of him, a degree of libidinally cathected reality which can compare with that of the hallucinatory, but to the patient immediately and vividly real, parent-images. And one finds evidence of it, of course, as has already been indicated earlier, in the parent's fighting tooth-and-nail, by every means at his or her disposal, against the patient's and therapist's collaborative effort toward the patient's becoming free from his

magically 'close', magically 'mutually understanding', two-against-the-world relatedness with the parent.

In what I have just said, no acknowledgment has been paid to the parents' contrasting, and healthy, desire to help their child reach true maturity, a fulfilment so at variance with the kind of subjectively mutually-omnipotent and celestially loving, but in actuality intensely ambivalent and psychotic, relatedness which I have described. Parents are never devoid of such a healthy parental desire and often, in my experience, that desire is sufficiently strong to enable them to make indispensable contributions to the endeavour in which patient and therapist are engaged. But it remains true, none the less, that this infantile-omnipotent relatedness between the 'sickest', least mature areas of the parent's personality on the one hand, and the patient's personality on the other hand, constitutes the greatest obstacle, in my experience, to the patient's becoming well.

All this becomes reproduced in the transference-development of an ongoing patient-therapist relationship, and the therapist inevitably becomes deeply immersed in the subjective experience of magical closeness and shared omnipotence with the patient. The enthralling nature of this phase accounts in many instances, I believe, for the great length of time consumed by the over-all treatment of these patients. The therapist gets at least one foot into the psychological process in which the patient himself is engaged, namely the process of maintaining a split between his 'good self' and his 'bad self', as well as a split between the 'good other person' and the 'bad other person'; generally, in fact, the therapist gets both feet into this process, for X number of months. Then both he and the patient spend much time basking in a purely 'good' experience of himself and of the other person, while the 'bad' elements in the relationship are maintained in a state of repression and projection on to the world outside the nest. The therapist, in experiences of this sort, learns at first hand how strong was the lure

offered to the patient in childhood by the parent, the lure to share the delights of being 'crazy' along with the parent.

(6) Something of the complexity which exists in an interpersonal relationship, the complexity of the true state of affairs which renders any attempt to describe that relationship (such as I am attempting here) a crude oversimplification, becomes evident when we consider this next motive.

A mode of interpersonal participation which bears all the earmarks of an effort to drive the other person crazy may be powerfully motivated, in actuality, by a conscious or unconscious desire to encourage the other person into a *healthier* closeness, a better integration both interpersonally, with oneself, and intrapersonally, within himself. In fact, successful psychotherapeutic intervention often takes on precisely this outward form.

Here, that is, the conscious or unconscious effort is to activate dissociated or repressed elements in the other's personality, not with the goal of his ego's becoming overwhelmed by their accession into awareness, but rather with the goal of his ego's integrating them. I do not mean, of course, that the initiator of this kind of participation conceptualizes all this, plans it all out in his mind in any such detail.

This fostering of the other person's intrapersonal and interpersonal integration or self-realization is a part of the essence of loving relatedness as defined by the philosopher-theologian Martin Buber (Friedman, 1955). He refers to this as 'making the other [person] present' and, when it occurs mutually, as 'mutual confirmation': and he expresses his conviction that '... The help that men give each other in becoming a self leads the life between men to its height...' (Limentani, 1956).

To put it in other words, it seems to me that the essence of loving relatedness entails a responding to the wholeness of the other person—including often (particularly in relating to a small child or to a psychiatrically ill adult, but to a lesser degree in relating to all other persons also) a responding in such

fashion to the other person when he himself is not aware of his own wholeness, finding and responding to a larger person in him than he himself is aware of being.

Thus, to focus again more specifically upon the seeming effort to drive the other person crazy, we find that this effort can be very close to, and can even be comprised of, an effort to help the other person toward better integration, which latter effort can be considered the essence of loving relatedness. A genuine effort to drive the other person crazy—to weaken his personal integration, to diminish the area of his ego and increase the area of dissociated or repressed processes in his personality—can be considered, by contrast, precisely the opposite of the kind of loving relatedness which Buber describes.

I surmise that in many instances of a parent's fostering his or her child's 'going crazy', the psychosis in the offspring represents a mis-carriage of the parent's wish, conscious or unconscious, to help the child toward a better, more mature integration. One cannot always know the precise ego-capacities of the other person, as any therapist can attest, and it may well be that parents often perform acts analogous with those interventions of a therapist which are ill-timed or otherwise ill-attuned to the needs of the patient's ego, and which have, instead of the desired effect of further integration in the patient, a disintegrating effect.

I think it significant in this regard that, in very many instances of the outbreak of psychosis, the precipitating circumstances, whatever they may be, have led the patient to become aware of truths about himself and his relationships with others in the family, truths which are actually precious and long-needed, truths which could provide the basis for rapid ego-growth, rapid personality-integration. But they come too fast for the patient's ego to assimilate them and the ego regresses, recoiling from what is now, in its effect, an opened Pandora's box. Thus what could have become—and what in probably a great many instances does become, in persons who never get to a psychiatrist—a valuable, creative,

integrative growth experience, becomes an experience of developing psychosis, as various pathological defences (delusions, hallucinations, depersonalization, and so on) become erected against the awareness of those truths.

In psychotherapy the therapist is often called upon to contribute, in skilfully dosed and skilfully timed increments, the very kind of participation which would, if given less skilfully (whether by reason of inexperience or by reason of his orientation toward the patient being a predominantly hateful rather than a predominantly loving one), have an effect precisely opposite to the therapeutically desirable one. For instance, premature interpretations may have a disintegrating rather than an integrating effect upon the patient.

(7) The next motive which I shall discuss can be seen in connexion with a point made prominently by Hill, the point that the mother of the schizophrenic keeps before the child the threat that she will go crazy if he becomes an individual by separating himself, psychologically, from her.

The relevant motive, then, is this: the child's own desire for individuation may be experienced by him as a desire to drive the mother crazy. The mother reacts to his desire for individuation as an effort to drive her crazy; so it seems to me entirely natural that the child himself should be unable to distinguish between his own normal and precious striving toward individuation, on the one hand, and on the other hand a monstrous desire—which latter the mother repeatedly reacts to him as evidencing—to drive his mother crazy.

Such a state of psychodynamic affairs is entirely analogous, I think, to that which obtains in a situation where the mother indicates that if the child really grows up, it will kill her; there, as one finds in clinical work, the child comes to experience his normal desires to become an individual, as being monstrous desires to murder his mother. Hill makes this latter point in his volume.

If, in looking a bit further into the child's relation with such a mother as Hill describes, we shift the frame of reference to visualize

the *struggle between* mother and child to drive one another crazy, another interesting point emerges: the mother's ostensible efforts to drive the child crazy can be seen as containing, probably, a nucleus of laudable motivation, though unformulated as such by her, to help her child become an individual. It is probable that, in the mind of such a mother, the concept of psychological separateness, of individuality, is to such an extent equated with craziness, that she cannot conceptualize this motive as a wish to help her child become an individual. But it may well be that some bit of healthy mother in her senses that the child needs something which she is not providing, something utterly essential, and that it is this part of her which ostensibly tries to drive the child crazy—tries, in actuality, to help the child become an individual.

In the psychotherapeutic relationship we find that, as a natural consequence of this past experience of the patient, he tends to react to his own developing individuality, his own ego-growth, as anxiety-arousing craziness; and the therapist (in the transference-position of the mother at this phase of the work) tends to experience this anxiety too. Thus both participants tend unconsciously to perpetuate a symbiotic relatedness with one another, out of mutual anxiety lest the patient 'go crazy' completely—lest, in truth, the patient emerge from the symbiosis into a state of healthy individuality. This formulation is in line with Szalita-Pemow's comment (in a personal communication) that 'The [schizophrenic] patient's individuality resides partly in his symptoms'.

(8) The final motive is actually, in my experience, most often the most powerful of all these motives; my mention of it at this juncture is brief because it has already been touched upon in discussing motive (5) above, and because so much of this paper's final pages will be devoted to it. This motive is the attainment, perpetuation, or recapture of the gratifications inherent in the symbiotic mode of relatedness. More often than not, the effort to drive the other person crazy, or to perpetuate his craziness, can be found to rest primarily upon both

participants' unconscious striving for the gratifications which the 'crazy' symbiotic mode of relatedness, despite its anxiety- and frustration-engendering aspects, offers.

I shall not attempt to discuss other possible motives behind the effort to drive the other person crazy. One can undoubtedly find others, and some of them may be of as widespread importance as those I have described. But the eight which I have mentioned are, in my experience at least, the most frequently occurring and powerful ones.

C. THE PATIENT-THERAPIST RELATIONSHIP

A considerable portion of the clinical experience which has led to this paper's central hypothesis has consisted in reports about, and observations of, patients' relatedness with their parental family-group and with the group of patients-and-personnel on their hospital ward; if space allowed, I could present data indicating that in each of these arenas, the patient's integration with the group takes the form, in many instances, of a mutual struggle to drive the other person crazy.

But, since I consider that fundamentally the same psychodynamic processes are at work there, the only fundamental difference being that they are there operative in a group, rather than dyadic, setting, and because my main interest is in individual psychotherapy, I shall confine my remaining remarks to the context of the patient-therapist relationship.

In my experience, it is in the patient-therapist relationship that one can discern most clearly this mode of interaction, the effort to drive the other person crazy. Specifically, one can find this type of relatedness predominating during one particular phase of the schizophrenic patient's evolving transference to the therapist, a phase in which there is reconstituted, between patient and therapist now, an earlier *struggle between patient and parent* to drive each other crazy. From my own therapeutic work, and from what I have observed of other therapists' work here at Chestnut Lodge, I have obtained the impression that any successful course of psychotherapy with a schizophrenic patient

includes such a phase. During it the therapist becomes, in most instances I believe, deeply involved in this struggle, such that he does indeed feel that his own personal integration is in real jeopardy of a greater or lesser degree. The therapist's necessary participation in this phase of the transference-evolution is one of the main elements in psychotherapy with schizophrenics which make this work at times so stressful to pursue.

One of my male schizophrenic patients expressed his conviction to me, for more than two years, that, as he put it, 'You're kind of *strange*, Dr Searles'; 'You're crazy, Dr Searles'; 'You think peculiarly'; and he would say knowingly, 'You don't express yourself to other people the way you do to me, do you?' In the development of this man's transference to me as a mother-figure, there was revealed beautifully the fact that, in earlier years, he had repeatedly tested his mother's sanity by leading the mother into various situations and then seeing whether she reacted in a normal, or abnormal, fashion. The mother, who had died a few years before the patient's hospitalization, had been a highly schizoid individual with whom he had been involved, over many years, in a typically symbiotic relationship. The other family members, highly prestige-conscious people, had maintained a barrier of protectiveness and scorn around the eccentric mother and this son, so like the mother in that eccentricity.

In my work with this man, the struggle to drive one another crazy was re-enacted with unusual intensity. He did an almost incessant amount of testing of me, such as he evidently had done in earlier years with his schizoid mother, in such a way as to bring out evidence to support his persistent suspicion that I was slightly, or more than slightly, cracked. He reiterated, for years, maddeningly bland stereotypes, labelling himself as thoroughly healthy and good, and myself as warped and evil, with a kind of eroding tenacity; and at times he picked at me in the same baiting, sarcastic, accusing way that his mother evidently had employed toward him, to such a

degree that I could scarcely make myself remain in the room. He accused me, time and time again, of driving him crazy;* after my having gone through a number of hours with him in which I had to struggle with unaccustomed effort to maintain my own sanity, it began to occur to me that this oft-reiterated accusation of his—that *I* was trying to drive *him* crazy—might involve some projection.

In the course of an hour with a 24-year-old schizophrenic woman I became assailed with feelings of confusion and unreality, when this patient, a luxuriantly delusional person, was reading to me from an instruction book concerning the Japanese game of 'Go'. She appeared to find some hidden meaning in almost every word and even in almost every syllable, looking at me significantly, with a sarcastic smile, very frequently, as though convinced I was aware of the secret meanings which she found in all this. The realization came to me, with a temporarily quite disintegrating impact, of how threatened, mistrustful, and isolated this woman was. What she was doing with me compares very closely with her mother's taking her to movies, during her childhood, and repeatedly commanding her, 'Now, *think!*', which the patient took—correctly, I believe—as the mother's command for the daughter to perceive the same secret, special meanings in the course of the movie which the mother, an actively psychotic person throughout the girl's upbringing, found in the picture. The patient had been quite unnerved by this impossible task (whose 'successful' accomplishment would have meant her sharing her mother's

psychosis), just as I felt unnerved during that session, with her reading. Also, on a later occasion she described her own having read to her mother in exactly such a fashion for hours on end while the mother was doing housework, and it was evident to me that she had derived much sadistic satisfaction from her own being able to drive her mother to distraction by that method. Many times, similarly, I saw her sit back with a triumphant smile after she had succeeded in making me thoroughly bewildered, and more than a little insecure, with her chaotic verbalizations of delusional material.

The woman I have just mentioned had been told over and over again, by various members of her family ever since she was a small child, 'You're crazy!'—whenever, as she herself remembered it, she pressed any one of them for information to resolve the confusion which, to some extent, all children experience often when they are exposed to unfamiliar and complex situations. She described it to me once that: 'Whenever I'd open my mouth, six or eight of them [i.e. other members of her unusually large family] would jump down my throat and tell me I was crazy, until I began to wonder whether I really was losing my mind.' It became quite clear that a mutual struggle to drive one another crazy had gone on between her on the one hand and other family members on the other hand. It had ensued with particular intensity between herself and her mother, an extremely changeable person (as corroborated by one of the patient's brothers) whom the patient was convinced, for years after beginning therapy with me, was not one person but many. Of this mother, she once made to me this statement, significantly indicative of the kind of struggle I have described: 'They used to say, "You're psychosomatic! If you don't watch out, you'll wind up in a mental hospital!" That's the way *they* were and they wouldn't admit it.'

There were hints in my work with this woman, however, that her efforts to drive me (as a mother-figure in the transference during this phase of the therapy) crazy were motivated

* A schizophrenic man, after several months of mutism, when he began talking to his therapist said, repeatedly, in anxious protest, 'You talk too queer. . . . You're too crazy.' Following a visit from his mother, he anxiously asserted that his mother wanted to kill him, and declared that his mother had made him sick and had driven the patient's brother (a monk) to the monastery. A few months later on, the same feelings came out in accusations to the therapist. 'You wanted to kill me. You made me sick. . . . crazy thoughts. You talk too queer.'

at times not primarily by sadistic pleasure in rendering me more or less disorganized, nor by a need to externalize upon me her own psychosis, but, rather, by genuine solicitude for me. At such moments the interaction between us was such as to make clear that I was in the position, as she saw me, of a mentally ill mother who needed treatment which she herself felt helpless to provide me—entirely similar to the situation which had obtained during her childhood, in her relatedness with a psychiatrically ill mother who never obtained the benefit of professional treatment for her chronic 'ambulatory' schizophrenia.

On one occasion when this facet of the transference was in evidence, the patient protested, late in a session in which we had been exchanging views quite actively: 'Why don't you go to a state hospital?—that's what you've been asking for, all the time you were talking.' She said this in a tone not of hostility but of solicitude and helplessness, as if she were being held responsible for placing me in a state hospital, and felt utterly helpless to carry out this obligation. In another hour two weeks later she demanded, 'When are they going to send you to a state hospital? . . . I know you're trying to get to one.' In these instances, I assume that one factor at work was a projection of her own unconscious desire to be sent to a state hospital. None the less, all this fits so precisely with the relatedness which had obtained between herself and her mother, and moreover there were such numerous additional indications that she was responding to me as being her mother from her childhood, that I was convinced of the above-described transference significance of her responses.

The above examples are predominantly illustrative of *patients'* efforts to drive their therapist crazy. My clinical material, from both my own therapeutic work and my observations of that of fellow therapists, indicative of the *therapist's* own comparable effort, suggests that therapists utilize (largely unconsciously, I again emphasize), just as do patients,

the whole gamut of modes or techniques which I have described earlier in this paper; and the range of underlying motives is seemingly as wide for therapists as for patients.

In any single instance, the therapist's striving in this direction can be found, I believe, to arise from two sources: (a) the nature of the patient's transference—namely, a driving-and-being-driven-crazy type of relatedness—to him, such that he is inevitably drawn, to some degree, into a state of feeling, and a mode of overt relatedness, which is complementary to that transference; and (b) a character-trait in the therapist, transcending his relationship with this particular patient, in the form of an unconscious tendency (of, undoubtedly, widely varying strength among various therapists, but probably not totally absent from the enduring constellation of personality-traits of any therapist), to drive the other person crazy—*whatever* other person, that is, with whom he establishes a significantly close relationship.

So then, when we find, upon examining any particular patient-therapist relationship, that the relationship at this stage is characterized predominantly by a mutual struggle between the two participants to drive one another crazy, it is probable that the therapist's behaviour of this sort is based partially—and, I think, in most instances predominantly—upon the first-described kind of 'normal' therapist-responsiveness to the patient's transference.

But in, I think, a significant percentage of such instances, the second of the sources which I have mentioned—sources for the therapist's behaviourally participating in this struggle—also plays a greater or lesser part. I discovered conclusive proof of such a character-trait in myself, to my great dismay, late in my personal analysis (about seven years ago)—a trait which I found to be in operation not only with regard to one or two of the patients with whom I was working at the time, but with regard to all of them, as well as with regard to innumerable other persons—relatives, friends, and acquaintances. The following general considerations are suggestive of a fairly wide distribution of

such a character-trait among psychotherapists and psychoanalysts:

(1) An obsessive-compulsive type of basic personality structure is certainly not rare among therapists and analysts. I am not convinced that such a personality structure predominates among us; but any one's informal observations suggest that it is probably to be found at least as frequently among us as among the members of the general population, in our culture which places so many premiums upon such obsessive-compulsive character traits as orderliness, competitiveness, intellectualization, and so on.

It is well known that one of the major defence mechanisms of the obsessive-compulsive is reaction formation. It should not be surprising, then, to find that the choice of a profession, on the part of a significant number of psychotherapists and psychoanalysts, has been founded partially on the basis of reaction formation against unconscious wishes which run precisely counter to the conscious endeavour which holds sway in their daily work. That is, just as we would not be surprised to find that a surgeon brings forth, in the course of his psychoanalysis, powerful and heretofore-deeply-repressed wishes to physically dismember other persons, so we should be ready to discern the presence, in not a few of us who have chosen the profession of treating psychiatric illness, of similarly powerful, long-repressed desires to dismember the personality-structure of other persons.

(2) To extend the line of—admittedly hypothetical—reasoning which has been presented in (1) above, it is understandable that in the training analyses of persons who have chosen psychotherapy or psychoanalyses as a profession, the analysand would encounter great resistance to the recognition, in himself, of such desires as those in question here—desires to drive other persons crazy—since these desires run so directly into conflict with his genuine and powerful interests in helping to *resolve* psychiatric illness. Hence such unconscious desires—such a personality-trait, that is—understandably might tend to escape detection,

and thoroughgoing resolution, in the training analysis, and the choice of a profession might never be revealed as constituting part of the analysand's struggle against his unconscious wishes to foster personality-distintegration in other persons.

I think all this might be described most accurately as follows: desires to drive the other person crazy are a part of the limitlessly varied personality-constellation of emotionally healthy human beings; therapists' and analysts' choice of a profession is suggestive that, at least in some instances where the personality-structure is of an obsessive-compulsive type, the individual is struggling against more-than-normally-strong unconscious desires of this particular kind; and finally, because therapists and analysts are engaged in the particular life-work to which they are devoting themselves (the relief of psychiatric illness), it is especially difficult for *them* to allow themselves to recognize the presence, in themselves, of these *qualitatively normal* desires.

(3) So many of us show a persistent readiness to regard this or that kind of functional psychiatric illness, or this or that particular patient, as 'incurable'—in the face of, by now, convincingly abundant clinical evidence to the contrary—that one must suspect whether this proclivity for the adoption of an unscientifically 'hopeless' attitude masks, in actuality, an unconscious investment in keeping these particular patients fixed in their illnesses. In raising this point, I do not wish to minimize the very great difficulties which stand in the way of recovery for many psychiatric patients; on the contrary, it is my first-hand experience with facing such difficulties, in work with chronically psychotic patients, that makes me feel it all the more important for us to bring into this formidable task as few as possible additionally-complicating factors of our own.

I have seen, by now many times over (in my work with chronically psychotic or neurotic patients, in my supervisory experience with approximately twenty other therapists at Chestnut Lodge and elsewhere, and in listening

to staff- or seminar-presentations by many additional therapists) how very prone we are to the development of an attitude of hopelessness, in the course of our work with a patient, as a means of unconsciously clinging to the denied, but actually profoundly valued, gratifications which we are obtaining from a symbiotic mode of patient-therapist relatedness. In this phase, we tend to fight tooth-and-nail, however unwittingly, against the patient's making a major step forward—a step which something in us senses to be in the offing. Time after time, a major forward move in therapy is preceded by such a phase of hopelessness on the part of both patient and therapist, a hopelessness which can now be seen, in retrospect, as a matter of their mutual clinging to their symbiotic mode of relatedness with one another.

There have been, by now, many articles and books written which emphasize the pathogenic significance of such a mode of relatedness in the patient's—especially the schizophrenic patient's—upbringing; but I think we have underestimated the intensely gratifying elements of that mode of relatedness, a kind of relatedness which allows each participant to luxuriate in feelings of infantile satisfaction as well as in omnipotent-mother fantasies. I think that one of the great reasons why schizophrenia is so difficult to resolve is that the therapist finds so much inner resistance against helping the patient to move out of the reconstituted patient-parent symbiotic relatedness in the transference. Not only the patient, but the therapist also, tends to find the prospective fruits of a more mature relatedness *just barely*—if even that—worth the relinquishment of the symbiotic relatedness which, despite its torments, affords precious gratifications also.

Whenever I have been able to acquire up-to-the-minute, detailed data about these situations I have found, as one might expect, that the mutual struggle between patient and therapist to drive one another crazy occurs on the threshold of what, as later events prove, is an unusually big forward step for the patient

in therapy. It is as though both of them fight, via a recrudescence of their mutual driving-crazy, symbiotic techniques, against the upsurge of this favourable step in the therapy.

I do not wish to leave the impression that the therapeutic road, after one such break-through, consists in nothing more than a straight, wide, smooth home-stretch. In the working through, in the transference, of the patient's symbiotic relatedness with the mother, this same struggle has to be gone through again and again. Although in subsequent repetitions it tends to occur with less disruptive severity, the therapist regularly finds himself susceptible to feeling the same black despair, the same sensation of being driven utterly mad by this impossible patient, time after time at the thresholds of successive stages in the loosening of the symbiotic relatedness. This may be compared with the foetus' becoming anatomically separate from the mother: not merely one, but a long series, of labour pains is necessary before the baby fully emerges. Of tangential interest, here, are the following remarks by Margaret Little in a paper entitled, 'Counter-transference and the patient's response to it' (1951).

Consciously, and surely to a great extent unconsciously too, we all want our patients to get well, and we can identify readily with them in their desire to get well, that is with their ego. But unconsciously we tend to identify also with the patient's super-ego and id, and thereby with him, in any prohibition on getting well, and in his wish to stay ill and dependent, and by so doing we may slow down his recovery. Unconsciously we may exploit a patient's illness for our own purposes, both libidinal and aggressive, and he will quickly respond to this.

A patient who has been in analysis for some considerable time has usually become his analyst's love object; he is the person to whom the analyst wishes to make reparation, and the reparative impulses, even when conscious, may through a partial repression come under the sway of the repetition compulsion, so that it becomes necessary to make that same patient well over and over again, *which in effect means making him ill over and over again in order to have him to make well* [italics mine—H.F.S.].

Rightly used, this repetitive process may be progressive, and the 'making ill' then takes the necessary and effective form of opening up anxieties which can be interpreted and worked through. But this implies a degree of unconscious willingness on the part of the analyst to allow his patient to get well, to become independent and to leave him. . . .

In my (1955) own experience, by contrast to that of Reichard & Tillman (1950), Lidz & Lidz (1952), Limentani (1956), and many other writers, I find that what the therapist offers the patient which is new and therapeutic, in this regard, is not an *avoidance* of the development of symbiotic, reciprocal dependency upon the patient, but rather an *acceptance* of this—an acceptance of the fact that the patient has come to mean a great deal, personally, to him. It is this acceptance of one's own dependency upon him that the mother had not been able to offer him.

I believe that in the great majority of instances where a patient and therapist have worked together for a long enough time for this symbiotic relatedness to become well established, and where we find that both are feeling hopeless about the work, we can find much evidence that each is unconsciously struggling to drive—or perhaps, more accurately, to keep—the other person crazy, so that he can cling to this highly immature and therefore 'sick', but deeply gratifying, symbiotic mode of relatedness with the other.

It may well be that the widespread need—widespread not only among schizophrenic patients but among, also, the professional persons who treat them—to deny the gratifying aspect of the symbiotic relatedness, accounts for some of the persistent viability of the irrational, name-calling 'schizophrenogenic mother' concept. That is, it may be that we are so powerfully drawn, at an unconscious level, toward the gratifications which such a mother offers, with her symbiotic mode of relatedness, that we have to deny our regressive urges in that direction, and this consciously perceive, and in scientific writings describe, her as a quite totally unappealing 'schizo-

phrenogenic mother' with whom it would be pure hell to relate oneself closely.

(4) So many therapists' and analysts' personally characteristic ways of responding to patients' communications sound, not infrequently, as if calculated to drive the patient crazy (or crazier), that it is difficult to attribute this phenomenon entirely to lack of clinical experience, skill, and perceptivity. That is, I surmise that many instances of awkward therapeutic technique, technique which fosters further disintegration rather than integration in the patient, may be due to chronically repressed (and therefore chronically present) desires in the therapist to drive the other person crazy.

As one frequently encountered example, we therapists have a strong tendency to react to only one side of a patient's ambivalent feelings. Thus when a hospitalized schizophrenic patient, for instance, is evidencing grossly disturbed behaviour such that we are given to know that he has an unconscious need for the security of continued hospitalization, but he is *consciously* expressing to us a strong, verbalized demand that he be allowed to move out, we may reply, 'I realize that you *really want to stay* in the hospital, and are *afraid of moving out*,' in a reassuring tone. This example involves a crudity of therapist-technique which one does not encounter with extreme frequency in quite so stark a form, although I have observed, not uncommonly—and retrospectively have realized that I was using in my own work with patients—just as crudely untherapeutic a technique as this. But lesser degrees of this kind of untherapeutic therapist-participation (throughout this paper, the points made are considered as applying in psychoanalysis as well as in psychotherapy, although with especial prominence in the latter) are observable with very great frequency indeed. Surely many a neurotic patient in analysis, for example, finds himself maddened on frequent occasions by his analyst's readiness to discount the significance of the patient's conscious feelings and attitudes and to react to pre-conscious or unconscious communications as

though these emanated from the only 'real' and 'genuine' desires and attitudes.

The therapist's or analyst's growing out of such ways of responding is not simply a matter of his learning a technique more appropriate to the patient's genuinely ambivalent, poorly integrated state. To become more useful to his patients he must in addition be prepared to face his own conflict between desires to help the patient to become better integrated (that is, more mature and healthy) and desires, on the other hand, to hold on to the patient, or even to destroy him, through fostering a perpetuation or worsening of the illness, the state of poor integration. Only this kind of personal awareness prepares him for being of maximal use to patients—above all, to schizophrenic and borderline-schizophrenic patients—and,

particularly, for helping them through the crucial phase of the transference which I have been describing in this paper.

ACKNOWLEDGEMENTS

The following therapists have given me their kind permission to use, here, data concerning their patients: Drs Cecil C. H. Cullander, Jarl E. Dyrud, John P. Fort, Leslie Schaffer, Roger L. Shapiro, Joseph H. Smith, and Naomi K. Wenner. For the theoretical formulations concerning these data, I alone am responsible.

This research was supported by a grant from the Ford Foundation to the Chestnut Lodge Research Institute. An abbreviated version of this paper was read at the Second Annual Chestnut Lodge Symposium, 31 October 1956.

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AN EXPERIMENTAL APPROACH TO THE PSYCHOPATHOLOGY OF CHILDHOOD: SLEEP DISTURBANCES

BY JAMES ANTHONY*

The game of murder, which many of you must have played at some time in the past, is designed to take place after dark in a house where all the lights have been put out. The game begins with the players drawing lots out of a hat to decide on their various roles. Only two of these are specified—the murderer and the detective. The remainder are potential victims who creep about in the dark until the murderer ‘kills’ one of them by touching him. The victim counts up to ten, lets out a scream, and ‘dies’. All the players then join the detective who sets about discovering the culprit by a process of cross-questioning. Everyone is committed to tell the truth except the murderer, and he is entitled to lie whenever it suits him to do so.

Children react to the game in a variety of subtle ways. Most are excited by it. Some play it without anxiety and enjoy playing it. Others want to play it but are unpleasantly apprehensive throughout it. Still others are afraid to play it and avoid it if it is possible (Murray, 1933).

When the daughter of the American psychologist, Henry Murray, planned a week-end country house-party of five girls—Jane, Mary, Lou, Jill and Nan, all aged 11—her distinguished father regarded it as a wonderful opportunity for testing his hypothesis that a functional relationship existed between emotional and apperceptive processes, so that a change in one was likely to be accompanied by a qualitative or quantitative change in the other. He therefore planned a programme for them which included motoring through the countryside (‘normal pleasure-invoking cir-

cumstance’) followed by the assessment of thirty photographs of men and women (extracted from the magazine *Time*) on a 9-point scale ranging from ‘goodness’ (generous, kind, loving, tender, etc.) to ‘badness’ (cruel, malicious, wicked, etc.), where 1 was extremely good, 5 was average, and 9 was extremely bad (a ‘projective’ test). After this, the girls played the game of murder (‘fear-invoking situation’) and then submitted again to the tests of the photographs. Table 1 summarizes the week-end schedule as arranged by Murray. It also includes a number of unplanned events which occurred unexpectedly during the course of the two days.

The most important of these unplanned events occurred early in the morning following the evening’s game of murder. Jane woke the household in a state of great fear and declared that for two hours she had watched burglars prowling about her bedroom and ransacking the drawers. Later she had heard the dogs chasing the men down the lane. On investigation, none of these alleged observations proved to be true, and it became clear that the girl had suffered from a hypnopompic hallucination. She herself insisted on the verity of her experience, and although at first the rest of the girls all disbelieved her story, two of them, Mary and Lou (who had been frightened by the murder game) gradually succumbed to the suggestion and refused to sleep in the ‘burgled’ bedroom. Jill and Nan, on the other hand, seemed quite unaffected, and demonstrated their unconcern by sleeping there.

In the test that followed, the difference between the susceptible and non-susceptible group was marked. The former judged the faces as much more malicious than before, whereas with the latter, the reverse was true.

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Table 1. *A week-end country house-party*

(After H. A. Murray.)

The events	Jane	Mary	Lou	Jill	Nan
Motoring in country	Enjoyed	Enjoyed	Enjoyed	Enjoyed	Enjoyed
First project-photo test (av. rating per photo)	7.00	5.40	5.13	5.53	7.27
The game of murder (2 games)	Victim and murderer	'Frightened to death' 'so scared'	Afraid	Enjoyed	Enjoyed
Hypnopompic halluci- nation of burglars	+	—	—	—	—
Belief in its verity	Complete belief	Inclined to believe	Inclined to believe	Disbelief	Disbelief
Refusal to sleep in room	Complete refusal	Complete refusal	Complete refusal	Occupied room	Occupied room
Sleep response (suggestibility)	Search for burglars under bed and in toilet, night light demanded and adult visits; voices kept low so burglars cannot hear			Dropped off soon and slept peacefully	
Second photo test	7.80	6.47	6.07	5.33	6.67
Difference in rating	+0.80	+1.07	+0.94	-0.20	-0.60
Personality predictions (hostess)	Susceptible	Susceptible	Susceptible	Non- susceptible	Non- susceptible
Later sleep disturbances	Nightmares (burglar content)	—	—	—	—

This was not the end of the story. For some months after, Jane suffered from nightmares about burglars, but unfortunately no further information is given about her subsequent history.

According to Murray (1933), the psychological sequence underlying these various events was as follows: the activation of fear. The perseveration of this activated fear over a period of time, and the mobilization of 'active unconscious imagery' with its projection on to the material provided. All of this added up to 'the complementary apperceptive projection subsequent to the activation of an emotional state'. This turgid phrase incorporated the several ingredients that entered into the genesis of Jane's sleep disturbance. There was the traumatic situation, the heightened imagery, the personality traits of fearfulness, emotionality and suggestibility which led to an accurate prediction of the test results, and the communication of fear from one susceptible

individual to another. But about the essential ingredient that differentiated the susceptible from the non-susceptible girls, Murray was in doubt and he speculated on its possible relation to the intensity of the emotion aroused, to the partial dissociation of the personality, or to *conglomeration*—a childhood phenomenon in which the image is confused with the object, and the internal with the external (similar to Piaget's *a-dualism*).

I read this important paper after I had completed my own work on sleep disturbances, and I was immediately struck by the similarity of our conceptual approach to what was basically the same problem, even though we approached it from opposite directions. Murray had begun with an investigation of imagery and this led on to the production of sleep disturbances (hypnopompic hallucinations and nightmares). I, on the other hand, had started with an investigation of sleep disturbances and had been brought, with seeming inevitability, to an

analysis of imagery. He searched for the essential ingredient among the excesses of personality, whereas I looked for it in the nature of the imagery. His susceptibles were defined on the criterion of a projective test; mine on the basis of a clinical disturbance of the sleep function. Had Murray been a clinician and not a psychologist, he would at once, I believe, have linked the disturbance of imagery to the disturbance of sleep, using as an intermediary the dream image which has both a psychological and clinical significance. As it was, he fell back on purely psychological explanations because he excluded the clinical data.

Being a clinician, the dream image loomed larger in my reckoning and furnished me with some initial concepts. Acting on these, I administered a card—The Dreaming Child, extracted from the M.A.P.S. series—to an experimental and control group of children, asking them to produce 'a real sort of dream' to go into the bubble. It was at once noticeable that the 'dreams' of the sleep-disturbed were more vivid, more real and more unpleasantly toned than those produced by the control sample of clinic children without a history of sleep disturbances, where the function of the dream to keep the child asleep, ascribed to it by Freud, was still intact.

Two further interesting facts emerged from this small projective study, intended as a pilot to the main one. First, the ability to manufacture artificial dreams to order appeared to have a fairly close relationship to the amount of night dreaming and day dreaming, suggesting some possible common source for the 'oneirogenic capacity'. Secondly, it was found that the patients who walked in their sleep did not dream so much or so vividly as patients whose disturbances took the form of night terrors and nightmares.

These two findings, taken together, prompted the further suggestion that the 'oneirogenic capacity' had its basis in the sphere of imagery, and it was to this group of phenomena that I turned to provide me with the operational concepts with which to conduct the major pro-

ject. I was well aware that this subject was still regarded in the best academic circles as dangerously subjective, not to mention old-fashioned, but I had always regretted that the best work on imagery had been thrown out with the mystique that unfortunately had followed in its wake, so that studies in perception had ceased to be respectable beyond the after-image. I was inclined to think, on intuitive grounds, that the perennial problem of symptom-choice had its roots somewhere in these border-line phenomena.

The rejection of this type of data is not always absolute. Some will accept them if elicited from adults or from adults trained in introspective techniques. The majority, without first-hand experience, will condemn its application to children in the belief that they are fundamentally incapable of such introspective appraisals, and they may even cite Piaget's authority for the view that the 'infantile realism' of the child and his liability to distorted, 'egocentric' perspectives render him a poor subject for such experimental procedures.

It is certainly true that he may 'conglomerate' the image and the object and confuse the insubstantial with the concrete, especially when the differentiation is never brought to his conscious notice, but no one has shown better than Piaget how fascinated the child can become in his imaginal productions, when indirectly approached in the spirit of play and made more self-conscious about them. He may not understand the phenomena, but he can be easily induced to report on them. His capacity for abstraction is not involved. The image is there if he can be coaxed to look at it, and being 'there', he can be stimulated to take an interest in it, as the Marburg school has so well shown.

The two principal postulates that arose out of such considerations were these:

(1) The differences in the manifestation of a sleep disturbance lay in the differences in the capacity for imagery.

(2) The capacity for night imagery was directly proportional to the capacity for day imagery, and, therefore, the assessment of day



imagery was also an indirect measure of night imagery.

Since we are now going to focus on the image as an explanatory manifestation, I shall attempt to describe in summary form some of the main components that enter into the imaginal process. As a central feature, Murray (1933) envisages a pool of images that lies beneath the surface of consciousness. This reservoir is maintained by the constant flow of perceptions. From time to time the deep unconscious images are mobilized by some appropriate stimulus and discharged into the external world in the form of phantasies, hallucinations or projected imagery of the eidetic or hypnagogic variety (Fig. 1).

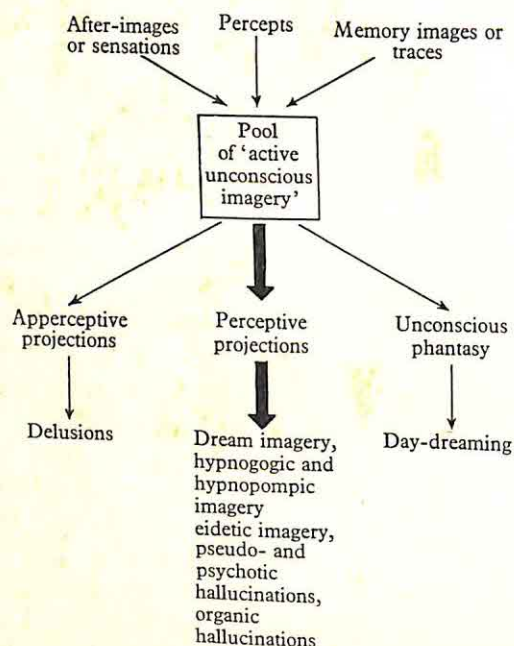


Fig. 1. The image in the outside world. (A fictional heuristic schema modified from H. Murray.)

In this paper, we are chiefly concerned with what Murray calls the perceptive projections and with their influence on symptom formation.

The image within the central system is a highly mobile structure that appears and disappears in different parts of the mental apparatus in seemingly random fashion. Its

circuit is probably governed to some extent by the functional capacity of the parts to deal with it. Where there is a superior aptitude, the image may tend to persist for abnormally long periods.

If we set out to trace the hypothetical course of the image on its circuit, our first station would be in the external world at the moment of perception, and our first concern the way in which the subject looked at the object. The technique of looking depends in part on the efficiency of the visual system, and to an unknown extent, on the psychological characteristics of the subject as an observer. It is difficult to assess the role of experience in this matter as opposed to 'constitutional' tendency. Some individuals seem 'born' visualizers. Others can be trained to look, but a certain small number apparently remain incurably 'haptic' (Revèsz, 1950).

Having impinged on the lens, the percept is brought to a focus on the retina as the first image, and this is followed under certain circumstances by the production of an after-image. The intensity and persistence of the latter seem to have little bearing on the intensity of other images on the circuit and are probably purely local phenomena. Somewhere on the journey, at a point not localizable, the image may become invested with perceptual qualities of extreme vividness and verisimilitude, and may be 'fixated' for varying periods. It is then referred to as eidetic. At some other point, it may leave the main circuit, and go into storage as a memory image to await mobilization by some appropriate stimulus. On the other hand, it may move into an apperceptive zone to be endowed with special characteristics that make it seem substantial and real (Piaget, 1929, 1956) or loaded with anxiety and terror to a degree that causes its eventual projection as a dream image, hypnagogic image or psychotic hallucination.

Such terminal projections, therefore, could be the end-products of a hypertrophied visualizing capacity, of an abnormal tendency to 'fixate' the image or 'reify' it to such an extent that it is confused with the percept, and finally

of a psychopathological need to externalize it because of some inner emotional disturbance. There may be other factors involved whose influence is less understandable, such as the special type of physical habitus described by Jaensch (1930). (In this last context, it is interesting to note that whereas the night terror and nightmare cases incline to an asthenic physique, the sleep walkers are well-built and athletic.)

With such considerations in mind, it is possible to construct a paradigm that links the percept to the disturbing image arising during sleep (Fig. 2).

The tests for differentiating the two tendencies, modified from Lowenfeld (1939) and Piaget (1956), are all based on the characteristics described. For example:

Test 1. The apprehension of the outer world is either objective and representational or subjective, impressionistic and emotional. Drawings may be either correctly proportioned or distorted in some significant part, e.g. boy hitting brother may show an exaggeration of the aggressive hand and a Lilliputian reduction of the brother, as in one case.

Test 2. Ability or inability to synthesize kinetic visual stimuli. A peephole over a mov-

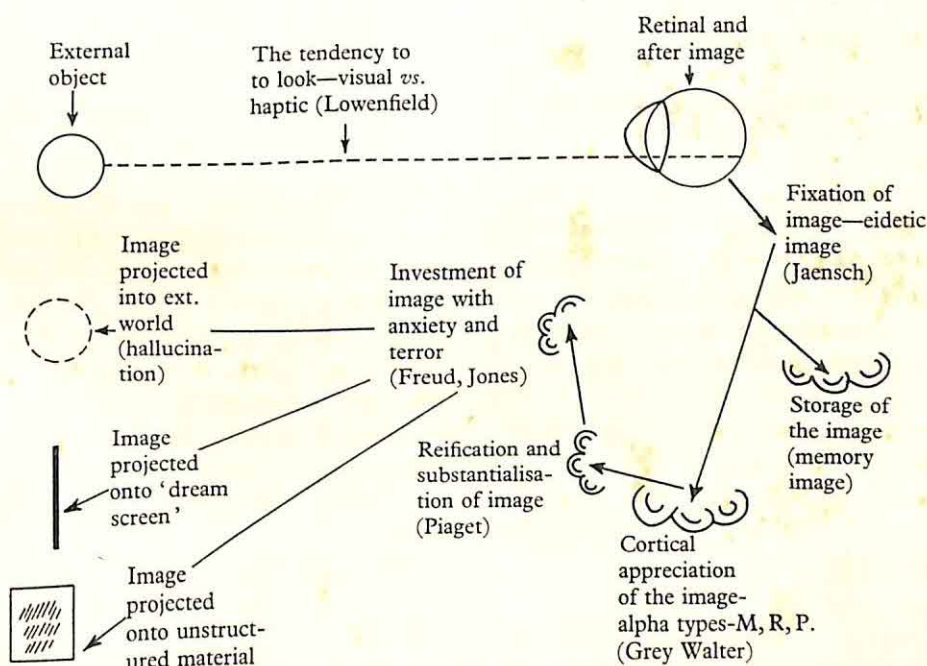


Fig. 2. Paradigm of the circuit of imagery.

Like many other functions, there is probably a normal distribution of the visualizing capacity through the population with extreme examples of visual and haptic tendencies situated at each 'tail' and responsible for pathological developments. The 'pure' visual or haptic individual is probably in a minority and easily recognizable, but the mixed forms may show either a predominance of one or other tendency. The characteristics, as described by Lowenfeld (1939), are fairly striking (Table 2).

ing card allows only part of a figure to be visualized at any given moment.

Test 3. Primary interest in the outer world or in bodily feelings, tactile and kinaesthetic. A word association test investigates whether the subject reacts in one way or the other, e.g. the stimulus word 'mountain' may elicit either the descriptive word 'beautiful' or the 'proprioceptive' word 'climbing'.

Test 4. The haptic is at home with the 'blind' situation, whereas the visual is lost.

Table 2. *Visual and haptic perceivers*

(After Lowenfeld.)

Visual types (47 %)	Haptic types (23 %)
Observers and spectators	Act as if partially blind
Perceive objectively	Perceive subjectively
Impressionistic	Expressionistic
Visual stimuli effective ('feel lost in the dark')	Haptic stimuli effective ('At home in the dark')
Primary concern with external world	Primary concern with bodily experience
Extravertive tendency	Introvertive tendency
Synthesises partial visual experiences	Content with partial visual experience—no synthesis
Representation of objects and space determined by reality—proper perspective	Representation of objects and space determined by needs and values—subjective emotional distortion
Transforms haptic into visual experience	Cannot easily transform haptic into visual experience
More liable to suffer from visual hallucinations	More liable to suffer from haptic hallucinations

A special card carries words (inciting to certain actions—hissing, whistling) distinguishable by touch like Braille lettering. The time taken to carry out the command is recorded.

Test 5. Conversion of stereognostic feeling into visual terms. Plastic figures manipulated 'blindly' in a bag have to be recognized among figures presented at the same time visually. Visiles can do it but not haptics.

Tests 2 and 4 differentiate well. Test 5 differentiates fairly well. Tests 1 and 3 differentiate poorly or not at all.

The eidetic image, when fully developed, is also easily recognizable, and this is especially so in children with emotional disturbances. It is a vivid and intense image although seldom confused with reality or exploited by the *eidetiker* as a talent. It may often pass unrecognized, but its unobtrusive use may govern the activities of crystal gazers and *idiots savants* with automatic memories (Drummond, 1926).

Its main characteristics are outlined in Table 3.

Allport (1924) has suggested that the normal

Table 3. *Eidetic imagery*

1. Considered by Jaensch as the 'ontogenetic source' of all images, and intermediate between sensations and images
2. It is localized 'out there' in space, and appears to be seen rather than imagined. It is sometimes as vivid as the actual perception, and is truer to the original than the memory image
3. It may last up to 20 minutes or more, and in some cases ('T' types) it is difficult to shake off
4. It may be diagnosed on criteria of intensity, duration and richness of detail, especially the minute reproduction of meaningless detail
5. Unpleasant spontaneous eidetic imagery may occur with emotional disturbances and cause 'day terrors'
6. Children are seldom aware they have this tendency and do not exploit its possession in any way
7. There are no sex differences
8. 60 % of children at 4 are eidetic; only 7 % of adults are eidetic. It disappears round about puberty. Generally, the younger the child, the more eidetic he is likely to be. There is a decrease in both numbers and intensity with age
9. Black children are more eidetic than white children

function of the eidetic image is to preserve situations for the child so that he may practise and perfect his responses. Unfortunately, like other developmental functions, this particular one can get out of hand and become the locus for pathological formations. Miller has referred to it as an 'undertone of the psychoses'. The question has been raised as to whether it can be held responsible for artistic talent manifesting itself at an early age, but concentration on drawing would probably cause the eidetic image to vanish. Paradoxically, many sleep walkers, possessed of poor imagery, carry out stereotyped and repetitive drawings in profuse fashion. In these cases, as Drummond has pointed out, it is often clear that the drawing is a mode of expression taking the place of absent visual images.

room would also be able to see the dream. The question arose in this project as to whether this particular perspective of childhood added to the terror of the night attack by adding reality and a third dimension to the dream image particularly during the waking and falling asleep periods.

The image projected on to unstructured material is seen at its best in the Rorschach test. The investment of anxiety is shown by the shading responses, the preoccupation with frightening figures, the references to damaged and decayed things, and the prevalence of stark colours like black and red, blood and fire. It is interesting to note that this tendency is at its height in the Rorschach at the age of seven which is also the peak age of incidence of the night terror-nightmare group.

Table 4. *Some developmental transformations*

Phenomena	1-4	5-8	9-12	13-16
Fears	Concrete	Fantastic	Social	Social/sexual
Dreams	Animals biting and eating	Ghosts, witches, giants, animals, suffocating	School friends, pets	—
Rorschach (Clob and F(C) shading)	—	Percepts morbid decayed broken. Threatening figures, witches, giants, supernatural	Gradual decrease in F(C) and clob. Deeper depression subsiding	—
Eidetic imagery	?	Intense	Moderately intense	Slight
Suggestibility	?	High	Moderate	Low
Animism	Everything invested with life and feeling	Everything invested with life and feeling	Only things that move or move by themselves	Very little animism

Adapted from Jersild, Ames, Jaensch, Binet, Piaget

The dream image appears quite early in childhood some time during the second year. According to Piaget (1929) it participates in the general *realism* belonging to the first seven years during which the child is said to suffer from the confusion of a-dualism. As a result of this, the dream becomes a concrete experience that comes into the bedroom at night to visit the child. Another person in the

Most of the tendencies so far described are transformed during the course of development, becoming qualitatively different or quantitatively less. There seems to be a general diminution of the imaginal processes that continues until puberty, when such tendencies as the eidetic disappear almost altogether, except in a few exceptional individuals. The genetic trends are summarized in Table 4.

We have now completed our examination of these special processes, and our next task is to see how far they cause or influence the disturbances with which we are concerned. Before doing so, however, I would like to make a few remarks about sleep disturbances in general.

The 'parasomnias', as Roger calls them (1932), can be classified in various ways, but a simple descriptive terminology will suffice us here (Table 5).

Table 5. *Classification of sleep disturbances*

(Clardy and Hill.)

TYPE 1. <i>Minor disturbances.</i> (Restlessness, mumbling, talking, teeth grinding, early or frequent waking, difficulty in falling asleep)	46 %
TYPE 2. <i>Nightmares</i>	7 %
TYPE 3. <i>Night terrors</i>	2 %
TYPE 4. <i>Sleep walking</i>	1 %
TYPE 5. <i>Nocturnal enuresis</i>	26 %

Various combinations may occur. De Sanctis described two varieties of a NOCTURNAL SYNDROME—epileptic (which includes the epileptic attack, nightmares, and hypnagogic hallucinations) and hysterical, this latter having an 'inner mental determination'

The night terror, as you see, is not at all a common condition. Its clinical characteristics are fairly striking. At some late hour in the night, the child is found in a bizarre, crouching posture in bed or rushing about in a state of great agitation and apprehension, screaming in terror, and staring at something in front of him with wide open eyes and dilated pupils. He is not fully awake, does not recognize people and is disorientated in time and space. He will, however, reply to questions and gradually respond to soothing suggestions of reassurance. In the morning he has no recollection of the nocturnal event. The attacks tend to resemble each other and to show a periodicity. For a week prior to the attack, there is an increased restlessness during sleep as if there was a building up of a disturbance to the point of discharge. The condition does not appear to be

related to other sleep disturbances, although it shares a similar background of anxiety, insecurity, instability, overstimulation and traumatization. In some cases, a definite sensory aura may precede the attack, usually taking the form of a nebulous visual image. Because of these various features, *pavor nocturnus* has been regarded as allied to the epilepsy-migraine group and a certain number of cases do show an E.E.G. record of epileptic type. Also, in known cases of epilepsy, epileptic night terrors may alternate with epileptic attacks.

A subjective experience of night terrors has been described by a young physician, Shackle (1928) from personal knowledge. He insists that night terrors should not be confused with nightmares and suggests that there is a very sharp dividing line between the two conditions. According to him, the chief distinctions between them are that a nightmare might occur on any night or even several times in the same night and is associated with remembered visual images. The night terrors, on the other hand, occurred only once in the same night and always gave warning of their approach. No visual image could later be recalled. Here is a description of an attack, given by this medical patient.

On going to bed after a perfectly normal day I usually slept soundly, but occasionally, without any apparent exciting cause, I would be conscious of a feeling of *malaise*. A small night light was left burning and, by the dim light of this, objects normally familiar took on a peculiar aspect. It is best described by saying that they looked larger than usual and possessed an uncanny hardness and what a photographer would call 'contrastiness' of appearance; Meryon's famous etching of the Morgue has exactly the grimness and intensity of black and white contrast that was characteristic of the aura of the night terror. There frequently appeared just before the onset of the terror itself, a phenomenon which I have since come to recognize as a type of fortification figure; it was often replaced by an image of black and white cross-hatching. At the end of about half an hour's continuous aura, the night terror followed without perceptible interval. All consciousness of surrounding objects was lost and an image appeared

which was peculiar to the night terror. Vague, ill-defined masses were seen, some enormously large and others excessively small, conveying a general impression of prodigious contrast. The sensation experienced can be but inadequately described as fear of the most paralysing and dreadful kind; no fear experienced in nightmares or in real life bearing any relation to it whatsoever. How long this lasted there is no means of gauging; it seemed an eternity. . . . The aura preceding each attack was so well marked that I never failed to recognize it, and, realizing what was about to happen, used to strive to keep 'awake', if the expression can be used in this case, and thereby stave off the attack.

Shackle had two true attacks of migraine at the age of 13, and after that remained free until the age of nearly 30 when fortification figures appeared unassociated with headaches on several occasions after a spell of hard reading. He sums up his experience by saying that it resembled a little 'brain-storm' on its own, and once it was over nothing ever remained to mark its passage. He insists that the visual images of the aura never took on a form of objects seen in real life. In this context it is interesting to find that McKellar & Ardis (1956) in their analysis of hypnagogic images conclude that they fall into two categories which they tentatively call 'perseverative' and 'impersonal'. Perseverative images can be readily related to past experience or recent events whereas the impersonal image had a 'strange and seemingly archetypal character' which seemed foreign to the personality of the subject.

The postulated connexion that I made between night and day imagery has a bearing on the next condition that I shall discuss. This was first described by Hensch, and later by Still (1900), under the term of 'day terrors'. A child whilst sitting quietly in a room or in the midst of his play would suddenly, without any apparent cause, begin to scream and look terrified. Sometimes he would declare that (imaginary) people were coming towards him, and he would stare at them as if hallucinated. Clinically his state was reminiscent of a night terror and the similarity was more than superficial since the two conditions could replace or alternate with

each other. Like the night terror, the day terror seldom lasted more than about fifteen minutes and during it, the child looked vacant and seemed unable to recognize people. Occasionally he would even react to familiar figures with dread. Again, as in the nocturnal syndrome, his personality was described in such terms as excitable, nervous, fanciful, imaginative, fearful, sensitive, suggestible and tense, the total impression given being of a 'nervous disposition'. The physical appearance was similar—pale, 'peaky' and puny—and the family history was tainted with neuropathy.

Without the help of the Freudian unconscious or the electro-encephalogram, Still argued that the two conditions were the day and night versions of the same basic state stemming from factors that did not appear in consciousness and from a non-convulsive epilepsy. Sleep was not a necessary condition for the production of the terror, and therefore dreams were not the essential stimulus. Once the identity of the day and night terrors was admitted, it was no longer sufficient to define the night terror as simply the child's nightmare, since similar attacks could take place during the daytime. It was therefore likely that the night attacks were not really 'sleep terrors' but 'waking terrors'. If they were not related to sleeping and dreaming, then they might conceivably be examples of 'masked epilepsy' or 'paroxysmal neurosis'. He compared the cortical motor discharge in epilepsy with the affective discharge in the terror states, presumably also from the cortex. If this neurological model was true, the far greater prevalence of night terrors over day terrors might well be related to the increased sensitivity of the higher centres during sleep.

Experimental night terrors

Still's shrewd speculations have considerable bearing on the work of de Manacéine (1897) on the experimental production of night terrors. De Manacéine began with an interest in the twilight or hypnagogic state that occurs spontaneously in certain subjects prior to falling asleep. This half-awake condition has its

own characteristic pathology such as 'night starts' and 'hypnagogic hallucinations', both of which might set a child screaming with fright but could not be held responsible for the night terror. De Manacéine suspected that the hypnagogic and terror states might be related, but she had not sufficient experience of the former to know how the association worked.

In order to make a more adequate study of the hypnagogic state, she decided to create an artificial one. After numerous experiments, she discovered that by waking certain children suddenly out of a deep sleep, she could precipitate a 'twilight' state of up to five or six minutes, which gave her enough time to investigate some of its characteristics. The most relevant of these for our purpose were the following:

(1) The deeper the sleep, the longer the hypnagogic phase.

(2) The younger the child, the longer the hypnagogic phase.

(3) The more nervous and unstable the subject, the longer the hypnagogic phase.

(4) The longer the hypnagogic phase, the easier was it to induce an experimental night terror.

The experimental night terror could be brought about in certain susceptible subjects (where susceptibility was a function of age, 'nervous disposition', suggestibility, etc.) during the hypnagogic phase by making a fear-provoking suggestion, e.g. that the house was on fire. There was much to support the view that the artificial and natural attacks were of an identical nature. The age of incidence in both ranged from 3 to 8 years. After the age of 8 it was very difficult to provoke an artificial attack. There was usually a complete amnesia on waking; very occasionally it was remembered as a dream with secondary elaboration. The cataleptic postures adopted by the children were also a feature of both the artificially and naturally occurring terrors.

De Manacéine had no doubt that she was dealing with a dissociated or hypnotic condition, and that suggestibility was the prime

characteristic, so that repetition tended to facilitate the occurrence.

If we compare the nightmare provoked by Murray with the night terror provoked by de Manacéine, we note at once four differences:

(1) The responding children differed in age: 3-8 years in de Manacéine's experiment, 11 in Murray's.

(2) In Murray's experiment, the fear-provoking stimulus was given before the onset of sleep, while with de Manacéine, it was given during deepest sleep.

(3) Murray's children could produce the content of burglars on waking. De Manacéine's children were amnesic.

(4) In de Manacéine's situation, arousal set the attack going. In Murray's situation, it terminated the attack.

Both sets of susceptible children are, however, nervous, suggestible and fearful. The night terror subject seems less phobic and less prone to counter-phobic ritualism perhaps because of his amnesic curtain. He cannot remember so clearly of what he is afraid.

The nightmare or *Ephialtes incubus* awakens the child completely with a feeling of suffocation and helplessness. He is at once well-orientated, able to recognize people and his surroundings, and can furnish the content of an anxiety dream. The dream imagery is vivid and realistic and the memory of it and the dread of it may persist throughout the day, and even render the child fearful at the prospect of going to sleep again, fearful of the dark, and prone to counter-phobic rituals. One of Sully's patients avoided the bedroom because it was 'so full of horrible dreams'.

The nightmare, in fact, appears to be an exaggeration of the anxiety dream, and is fairly common. It is frequently associated with other sleep disturbances. Experiences of the previous day furnish the content ('perseverative images' to use McKellar's phraseology), and on analysis these tend to disclose typical neurotic conflicts. The attacks are not periodic and there is no association with epilepsy.

With sleep walking, we once again seem to be concerned with a dissociated state. The

somnambulist has his eyes open but is unable to recognize people. He is extremely suggestible and will maintain bizarre postures imposed upon him. His walk may last anything up to half an hour, and when he wakes in the morning he is amnesic about the night's events. The suggestibility may be extreme. Clerici (1930) reports a whole family going out for a sleep walk! Seashore (1916) speaks of it as 'the dream in action'—the dream is acted out rather than seen or heard (Roger, 1932). De Morsier (1931) compares the condition to twilight states, automatisms, dual personality, all of which involve a transitory amnesia.

I have summarized clinical features of the parasomnias in Table 6.

with flexible use and a disregard of minimal deviations, they were able to furnish a satisfactory control series.

The function of sleep has engaged the attention of theorists for many centuries. The older chemical and neural theories showed more interest in what put the sleeper to sleep than in what brought him out of his sleep into a state of wakefulness. This was reflected in their preoccupation with the existence and location of a 'sleep centre'.

Kleitman's evolutionary theory (1939) was in many respects more satisfactory than those of his predecessors, in that it took into account the whole cycle of sleep and wakefulness, and substituted for the hypothetical 'sleep centre'

Table 6. *The parasomnias*

Clinical aspects	Night terrors 4-7	Nightmares 8-11	Sleep walking 12-16
Ages of incidence	Uncommon	Common	Uncommon
Frequency	Under $\frac{1}{2}$ hour	Minutes	$\frac{1}{4}$ - $\frac{1}{2}$ hour
Duration of attack	Deep	Light	Deep
Depth of sleep	'Half awake'	Fully awake	'Half awake'
Degree of arousal	Wild terror	Suffocating fear	Calm
Affective state	Visual hallucination	Anxiety dream	'Dream in action'
Basic phenomenon	Present	Absent	Present
Cataleptic features	Present	Absent	Absent
Sweating	Marked	Orientated	Disorientated
Orientation on waking	Disorientated	Complete	None
Recognition of persons	None	Fully remembered	Complete amnesia
Memory of attack	Complete amnesia	Fully recalled	None recalled
Content of attack	None recalled		

The sleep curve in relation to the sleep disturbance

A proper understanding of sleep disturbances must presuppose some understanding of the function of sleep and the qualities of undisturbed sleep. The latter was important in the selection of control cases for our project. Our criteria for undisturbed sleep included easy induction of sleep (no insomnia), sound sleep (no parasomnias or motor restlessness), continuous sleep (no waking during the night or waking early), and finally, beneficial sleep (no waking up tired, lethargic and irritable). If we had applied these too stringently, all our control cases would have been eliminated, but

an equally hypothetical, but more reasonable 'wakefulness centre', situated somewhere in the hypothalamus and kept going by a stream of stimuli from an active cerebral cortex which is itself subjected to constant sensory impingement. A diminution in the latter leads to a diminution in the former with a consequent lessening in the activity of the 'wakefulness centre'. The individual is at first less awake, and then asleep. The 'height' of wakefulness varies as much during the day as the 'depth' of sleep does during the night.

To explain the development of the parasomnias, Kleitman postulates a block in the

cortico-hypothalamic stream, leading to cortical dissociation. As a result, lower cortical centres, usually only active in states of dreaming, etc., take over the stimulation of the 'wakefulness centre', bringing about 'an apparent state of wakefulness' which is the hypnagogic phase conducive to night terrors and sleep walking. Catalepsy, automatism, suggestibility and amnesia are phenomena of this secondary circuit.

The depth of sleep curve varies from individual to individual, and from season to season, but certain broad patterns emerge on analysis. These have been described as the classical curve (with a single deep trough an hour or two hours after the onset of sleep), the semi-classical or 'dromedary' curve, (with two troughs at about the second and the sixth hour), and the Kleitman curve in which a large number of secondary and tertiary curves are superimposed on the primary one. It is to some extent true that the sleep curve varies with the techniques used to assess it (E.E.G., autonomic measurements, sensory thresholds, body temperature, P.G.R. and motility) but my rough impression, after a limited use of the motility method with children, is that the Kleitman curve applies mostly to adults and that the classical curve, and especially the 'dromedary' curve (which figured in all our assessments) are the common patterns of childhood.

We are now in a position to localize the various sleep disturbances on the depth of sleep curve, realizing that the different disturbances require different degrees of depth for their expression. The cry of terror in the night can lead to the following diagnoses:

(1) A waking terror, prior to the onset of sleep brought about by visual illusions in the predisposed child lying awake in the dark.

(2) A half-waking terror occasioned by hypnagogic or hypnopompic hallucinations occurring during the twilight phase between sleeping and waking. Again, this is characteristic of certain children with a special tendency to fall into hypnagogic states.

(3) A sleeping terror occurring during the two periods on the curve of light and moderate

sleep, when anxiety dreams and nightmares have their greatest incidence.

(4) A sleep-wakefulness terror occurring when predisposed individuals are roused from very deep sleep into half-wakefulness. Sleep walking and epileptic night terrors are also brought about in the same manner.

The distribution of disturbances is given schematically in Fig. 3.

Having so far established the nature and identity of the various sleep disturbances and localized them on the depth of sleep curve, I shall now set my own sample of sixty-six sleep cases against this theoretical background.

The first striking feature to manifest itself is the clear-cut genetic trend in the age distribution as shown in Table 7.

This trend must be seen in relation to the developmental transformations previously discussed. Its significance will be discussed at the end of the paper but at this point it will be enough to comment on the gradual diminution in imaginal capacity and the gradual change in the way sleep disturbances manifest themselves.

The experimental cases were all taken in sequence from the clinic waiting list as they occurred. The hospital clinic is such that only major disturbances would be referred, the minor ones being presumably successfully dealt with by the general practitioners.

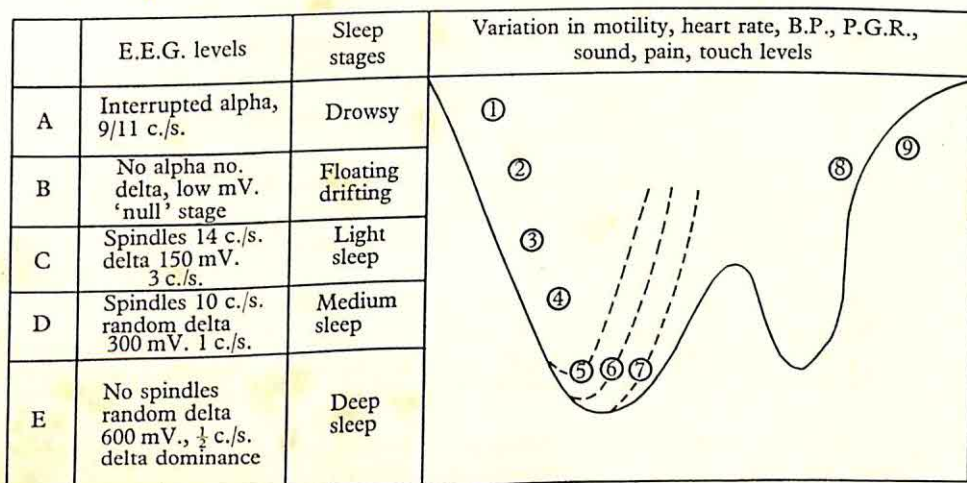
One intentional bias lay in excluding any mixed cases exhibiting both nightmares and sleep walking. There were five of these cases excluded from the sample investigated. The rest seemed to be fairly 'pure' examples of the three main groups. The sex ratio (girls:boys) varied with the age group; approximately 1:2 in age-group 1, 1:4 in age-group 2, and 1:20 in age-group 3. The relatively high incidence of girls in age-group 1 is mainly confined to the night terror cases, in which they predominated. The low incidence of girls in age-group 3 reflects their almost complete absence from the sleep-walking group. The reason for this interesting distribution is obscure, and it is as well not to lay too much emphasis on it since our sample was by no means a random one, and

there may have been many unknown biases at work in the referring system.

The control cases, about which something has been said, came from the general clinic and were not in any sense a normal sample except for the attribute of normal sleep. Since our

children suffered from disturbances of function other than that of sleep. (About half had eliminative disorders—enuresis and encopresis (cf. Anthony 1957)—and the remainder, disorders of feeding and motility.)

Clinically, the sleep-disturbed group, as a



- ① Night fears (illusional)
- ② Hypnagogic hallucinations and sleep 'starts'
- ③ Stage of dreams
- ④ Nightmares
- ⑤ Night terrors
- ⑥ Sleep walking
- ⑦ Epileptic night terrors
- ⑧ Hypnopompic hallucinations
- ⑨ Epileptic attack

= classical curve
 = semi-classical
 = Kleitman

Fig. 3. Depth of sleep in relation to sleep disturbances.

Table 7. Age patterning in a sample of children with sleep disturbances

Type of disturbance	Age-groups			Totals
	4-7	8-10	11-14	
Night terrors	18 (63 %)	7 (24 %)	5 (13 %)	30
Nightmares	3 (19 %)	11 (69 %)	2 (12 %)	16
Sleepwalking	1 (5 %)	6 (30 %)	13 (65 %)	20
Totals	22	24	20	66

$N=66$.

primary aim was the investigation of symptom-choice, we wanted to compare neurotic children with sleep disturbances with neurotic children without sleep disturbances, the neurosis being the common denominator. All the control

whole, showed many of the features of temperament and personality that we have mentioned in the general discussion. There was also a similarity of background, the children coming frequently from 'high tension' homes,

where they were overstimulated but given little opportunity for adequate discharge. Parental pressures, in the social and educational sphere, were often extreme. Each of the syndromes had an idiomatic symptom. Sleep rituals and night fears in the cases of nightmare, spontaneous hypnagogic hallucinations and day terrors in the cases of the night terror. In about one-third of the cases of sleep walking the children had suffered from tics at some time in the past or present.

Clinical hypothesis

A great many claims have been made with regard to the causal significance of certain factors in the child's history and environment. Most of them have in common a disturbed family background from which a chain of circumstances lead eventually to the expression of sleep disturbances. The sequence is demonstrated in Figure 4.

The clinical hypotheses from this theoretical background were as follows. It was predicted

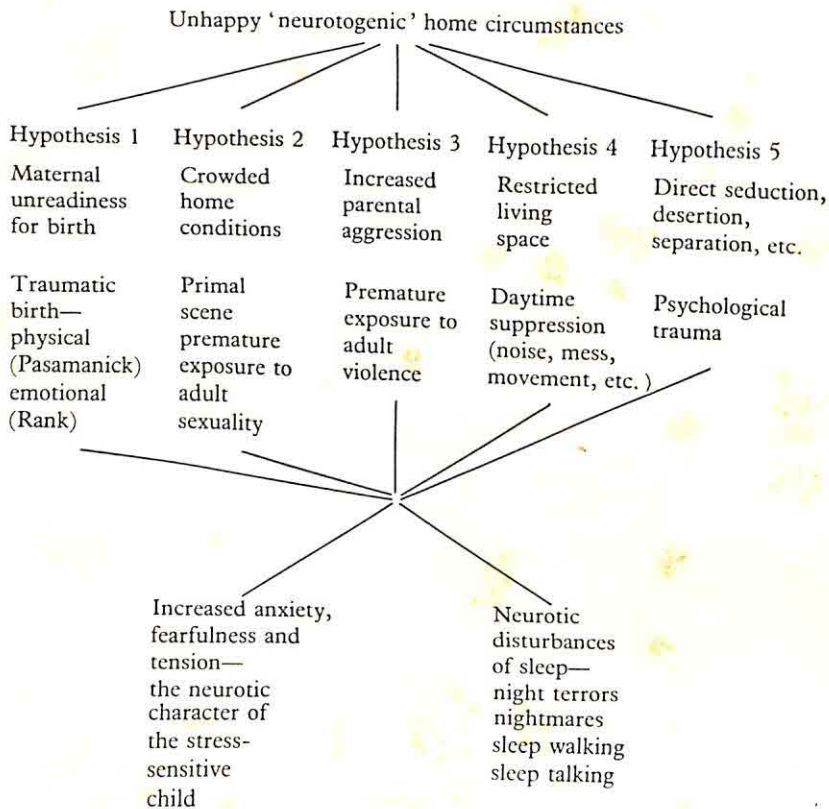


Fig. 4. Possible chains of circumstances.

It is not intended in this paper to explore the psychodynamic aspects of these cases, but in general one would say that they did not seem to vary in any significant detail from the neurotic controls. Oedipal disturbances, masturbatory guilt and severe aggressions bound up with sibling jealousy, etc., were seen in both groups.

Three main sets of hypotheses were tested by the clinical, psychological and experimental data.

that the experimental group would be differentiated from the control group by:

(1) Prolonged exposure of the child to the sexuality of the parents, as judged by the length of period of sharing the parental bedroom.

(2) Prolonged exposure of the child to aggressive conflict between the parents.

(3) The undue suppression of day-time activities.

(4) The experience of emotional and physical traumata; separation, hospitalization etc.

(5) The possession of a highly anxious, phobic and fear-communicating mother.

(6) The incidence of sibling birth at the onset of the neurotic illness.

(7) By the degree of mental illness in the family.

(8) By undue child-rearing pressures exerted by the parents.

The results of this analysis are given in Table 8.

It will be seen that there are two highly significant features. The first of these however—the duration in the parental bedroom—may

cation of sexual fears. It came out strongly in group sessions conducted with the mothers of the sleep disturbed children. With few exceptions, they revealed states of gross sexual maladjustment, and within this setting they manifested strong irrational fears of penetration, pregnancy and parturition. One girl of 8 remarked that she was *never* going to get married 'because lots of ladies were killed by having babies—Mummy said so.'

The trait hypotheses

Most observers have agreed about the incidence of a 'nervous disposition' among cases with major sleep disturbances, but there is some

Table 8. *Possible clinical factors in causation*

(Differences between the sleep-disturbed group and neurotic controls.)

Alleged cause	S-D group (%)	N.C. group (%)	Sig.
Duration parental bedroom over two years	35	16	0.01
Interparental aggression	43	39	N.S.
Suppression day activities	26	20	N.S.
Trauma (separation, etc.)	64	40	0.05
Phobic mother	73	34	0.01
Sibling birth	23	25	N.S.
F.H. of mental illness	19	22	N.S.
Undue parental pressures	39	33	N.S.

$N = 104$.

not measure the risk of exposure at all since it was sometimes difficult to decide whether the child was moved into the parent's bedroom before or after the onset of the sleep disturbance. This movement was sometimes so complicated that we referred to it as 'musical beds'. The child would move into the father's bed and father would move into the bed vacated by the child and this was permuted in various ways through the days of the week.

The high significance of the phobic mother obtained support from a comparison of the fear check-lists filled up by mother and child in which a correlation of 0.35 was elicited. This improved to 0.48 if only items with a strong sexual flavour were included. There was more than a paper demonstration of this communi-

measure of disagreement on the constellation of traits that contribute to this disposition. The occurrence of traits was tested in various ways, involving the use of check lists (for 'fearful' and 'tense') and profile recognition (for 'excitable', 'tense', 'emotional', 'fearful') in which both parents were given diagnostic profiles and asked to rate their child on a 7-point scale of being very much like the profile or very much unlike it. The parents carried out their ratings separately but simultaneously, so that no contamination was possible. The father-mother correlations were as follows: excitable 0.71, tense 0.23, emotional 0.68 and fearful 0.87. In general, fathers seemed to prefer the extreme ends of the scale, whereas mothers concentrated their judgements around the mean. The carbon

pressure test (less effective than the check list and profile recognition) measured the amount of pencil pressure transmitted through layers of carbon when carrying out some writing and drawing. In the ink blot suggestion, five unlikely projections were suggested to the child, about which he could be certain, doubtful or rejecting. For the test of imagination, the child was blindfolded and asked to describe what he could 'see'; and for the open-end stories, he was given a fantastic key situation (e.g. he was able to fly) and asked to carry on the story. It was especially interesting to note how quickly certain children lost their wings and came down to earth. The data on this section are summarized in Table 9.

(4) In the frequency, recency and vividness of their dream images (Jersild).

The results of these investigations are given in Table 10. In Figs. 5-7, the mean scores have been plotted against age.

The figures dealing with eidetic imagery and dream realism corroborate the trend of genetic depreciation described by others. The major differences lie between the night terror-nightmare group and the sleep walking group, the neurotic controls occupying an intermediate position. On the eidetic scale, there is a tendency for the large early differences between the three groups to diminish by the time puberty is reached. The visualizing capacity, on the other hand, tends to hold up. There is very

Table 9. *Trait hypotheses*

(Differences in reactivity between the sleep-disturbed group (S.D.G.) and the neurotic control group (N.C.G.).)

Trait hypothesis	Test	Sig.
S.D.G. more excitable	Profile recognition	N.S.
S.D.G. more tense	Profile recognition. Check list	0.05
	tension habits. Carbon pressure	
S.D.G. more 'emotional'	Profile recognition	N.S.
S.D.G. more fearful	Profile recognition. Check list by	0.01
	mother and child. Fear stimulus	
S.D.G. more egocentric	3 mountain test (Piaget)	N.S.
S.D.G. more suggestible	Ink blot suggestion	0.01
S.D.G. more imaginative	Griffith's method. Open-end	0.10
	story	

$N=86$.

Experimental hypotheses

The experimental and control groups were tested for eidetic, visualizing, substantializing and dreaming capacity. For the eidetic test, the technique described by Allport (1924) was used.

The following hypotheses were investigated. The experimental group, it was predicted, would differ from the control group:

- (1) In their greater tendency to use visual images (Lowenfeld).
- (2) In their greater tendency to fixate the image (Jaensch).
- (3) In their greater tendency to substantialize and reify the image seen (Piaget).

little difference at any stage between the three groups with regard to dream realism, although there is a suggestion that the sleep disturbed cases hold on to the concept a little longer in all age-groups.

Some incidental test anomalies

The detailed analysis of E.E.G. records has not been included for two reasons. First, only half the sample could be persuaded to undergo the examination. The remainder were too fearful—or else their mothers were, giving a 53 % refusal rate as compared with the 13 % refusal rate of the controls. Secondly, the interesting and very relevant alpha trends described by

Grey Walter and Short, that could classify subjects into visual ('M' types), mixed ('R' types) and non-visual ('P' types) did not show up in the records. This was not surprising since they are not thought to make any definite appearance before adolescence. Three of the sleep walkers were 'P' types, and three of the nightmare cases were 'M' types, and on the whole the sleep walkers had bigger and better alpha records than the others in the experimental

a whole and to a statistically-significant degree (average verbal I.Q. 84; performance I.Q. 109) manifested a striking verbal-performance discrepancy on the W.I.S.C. This would fit in with the general tendency of this group to be inarticulate and generally non-communicative.

CONCLUSIONS

The main conclusions to be drawn from this study are that the choice of symptom in this

Table 10. *Visual tendencies*

Test reaction	NT (%)	NC (%)	Sig	NM (%)	NC (%)	Sig	SW (%)	NC (%)	Sig
Strongly eidetic	88	64	0.05	81	44	0.05	35	40	N.S.
Strongly visual	68	32	0.01	75	56	0.10	25	50	N.S.
Marked realism	72	68	N.S.	25	19	N.S.	10	5	—
Intense dream imagery	68	56	N.S.	71	53	N.S.	15	55	0.01
<i>N</i> = 132	Mean age: 7.6			Mean age: 8.9			Mean age: 10.7		

NT = night terror cases; SW = sleepwalking cases; NM = nightmare cases; NC = neurotic controls.

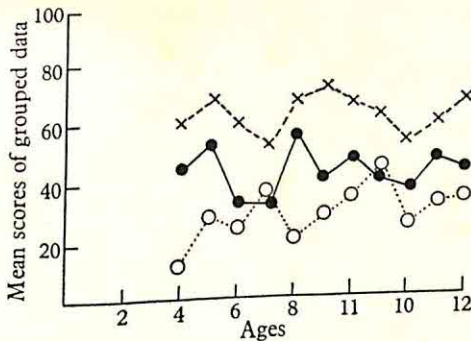


Fig. 5. Visualizing capacity (Lowenfeld). ---, night terrors; —, neurotic controls;, sleep walking.

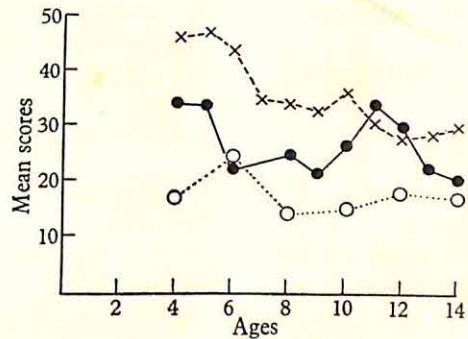


Fig. 6. Eidetic imagery (Jaensch). ---, night terrors; —, neurotic controls;, sleep walking.

group. General features of interest included five definite epileptic records—three from the night terror group, and two from the sleep walkers. In 20 % of cases, there were high voltage lambda waves, which have been associated with the occurrence of hallucinosis. In 25 % of cases the normal K (arousal) complexes were sufficiently marked to obtain a special mention in the report.

Another curious finding came from the intelligence testing. The group of sleep walkers as

area is governed by factors other than environmental and psychological. The latter are more concerned with the content of the disturbance and the general predicament of the individual. Choice is therefore a matter of psychophysical responsiveness. When the visualizing apparatus is hypersensitive, the individual will discharge into visual pathways and when the motor apparatus is hypersensitive the individual tends to discharge along the motor pathways. This basic tendency will determine in any

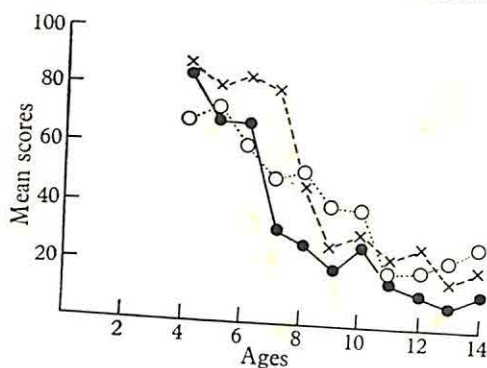


Fig. 7. Dream realism (Piaget). ---, night terrors; —, neurotic controls;, sleep walking.

given disturbance during sleep whether the patient will hallucinate a visual image or take to his feet. The developmental patterning of

disturbances is clearly related to the developmental patterning of the imaginal processes, and it is therefore not surprising that sleep walking tends to appear at the lowest point of the imaginal curve. When, however, the imaginal capacity persists, night terrors will persist into adult life. Most of the sleep disturbances can now be subsumed under the two main headings of visual and motor and the main characteristics of these two syndromes are summarized in Table 11.

I cannot refrain from concluding with an aphorism from Galen, the great physician, which runs as follows: 'Always this is to be remembered that no cause is efficient without an aptitude of the body'. It is this 'aptitude of the body' that the present investigation has attempted to demonstrate.

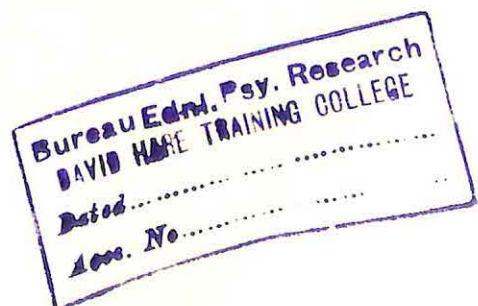
Table 11. *The two nocturnal syndromes*

Differential criteria	Visual syndrome	Mixed state	Motor syndrome
Psychopathology	Dissociated N.T., anxiety N.M.	Mixed	Dissociated state
Night symptoms	N.T., N.M., hyp. halluc.	N.T., N.M., S.W., Ep.	S.W.
Day symptoms	Day terrors, phobias	Mixed	Tics, stammer, restlessness
Development	Visual tendency	Mixed	Motor tendency
Interests	Visual arts	Mixed	Crafts, athletics
Lowenfeld battery	Strong visualizers	Mixed	Haptics
Jaensch	Strongly eidetic	Moderately eidetic	Weakly eidetic
Grey Walter	'M' type, no alpha	'R' type alpha with blocking	Persistent alpha
Dream imagery	Frequent, vivid	Intermediate	Infrequent, vague
Intelligence test	V better than P	V = P	Large P-V discrepancy

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RECENT MAZE TEST STUDIES

By S. D. PORTEUS

The publication in 1958 of the Porteus Maze Extension Series by the Centre de Psychologie Appliquée and the increasing use of the tests, both in Europe and America, makes it desirable to inform psychologists of any new developments bearing on Maze validity and reliability. Several recent studies, as yet unpublished or not readily available for reference, are therefore briefly summarized in this present article.

One of the most important of these was an investigation carried out by Smith (1958*a*) at Rockland State Hospital, New York, and reported by him in a paper delivered before the Eastern Psychological Association, U.S.A., in April of this year. Its title was 'Changes in Porteus Maze scores of brain-operated schizophrenics after an interval of eight years'. It will be published in due course.

Those who have followed the literature on psychosurgery may remember that the first Columbia-Greystone project (1949) was concerned with the mental effects of topectomy, or selective ablations of the frontal cortex. The second project (1952) was related to a variety of neurosurgical procedures, particularly thalamotomy; two types of venous ligation of frontal lobe blood vessels, one more caudal in situation than the other; thermocoagulation of areas 9 and 10 and transorbital lobotomy. The third project (1956) was a continuation of the Columbia-Greystone plan of research entitled The New York State Brain Study. Its patients were all schizophrenics who had suffered either orbital or superior cortical ablations. There were 45 operates and 33 controls and they were divided into two groups according to age and symptom complex. The younger group had a mean age of 34 years and were primarily retarded hebephrenic cata-

tonics, while the other group averaged 50 years of age and were mainly of the paranoid type. It was these New York Brain Study patients who were the subjects of Smith's investigation eight years later.

The results of all three of the earlier projects agreed in showing definite deficits in Porteus Maze scores after operation amounting to 1.21 years in the first project, 1.50 years in the second and 2.21 years in the third. All the control groups showed increase due to practice effects in the test.

In the second project, thalamotomy and the more posterior or caudal venous ligations brought about the greatest Maze impairment; transorbital lobotomy the least. In the third project, the site of operation had also a differential effect. The average loss for the superior cortical ablations was 2.88 years of test age as compared with 1.43 years for the orbital cases.

The only other test which showed a consistent loss after operation was the Capps Homograph test, though on the basis of ability to improve with practice, four subtests of the Wechsler-Bellevue Scale (Object Assembly, Digit Symbol, Arithmetic, Similarities) were significantly different. The verbal subtests—Information, Comprehension, Digit Span, Rote Memory and Vocabulary—were not affected to any significant degree by the operation.

Sheer (1956), in reporting these results, unfortunately used different measures of change. The mean Maze changes were given in terms of test age (2.53 years), the Wechsler-Bellevue Performance Scale in I.Q. points (5.17 points). This could be misleading, since when the Maze scores are converted into I.Q. points the mean loss was 18 points, or more than three times the Wechsler-Bellevue deficit.

* Manuscript received 20 September 1958.

In the third Psychosurgery Research Conference held in New York in 1951, Prof. Carney Landis stated that in his opinion all losses were temporary. This was based on the fact that in a year's time all patients, except the thalamotomy and posterior venous ligation cases, had made up their deficiencies in score, and that these losses should, therefore, be considered transient. In answer to my objection that the practice effects in a repetition of the Maze might conceal actual deficits in performance, Prof. Landis agreed that if a practice-free series of mazes could be developed and these showed a decline below pre-operative levels after a year, the losses would then be considered permanent. This statement was the stimulus for the devising of the Maze Extension Series in 1955.

But by this time the incidence of psychosurgical operations in U.S.A. had fallen to a minimum, largely owing to the use of tranquilizing drugs. There was, therefore, no opportunity for demonstrating permanent maze impairment by the use of the practice-controlled tests. However, Smith's investigation, to be described hereunder, has provided conclusive evidence that the decrements in Maze scores after psychosurgery are not only permanent, but that the mental defects increase with age.

Altogether there were available for re-examination nearly 9 years after operation 27 of the original patients and 23 of the controls. Presumably the use of controls was an attempt to balance the losses which resulted from operation against deficits that might be expected to occur in ordinary psychotics on account of increased age or other factors. Though the present writer is fully aware that comparison with controls is usual, he is very doubtful that such corrections are of any value unless the controls can be matched with the experimental group in such factors as rate of ageing processes; a procedure which is manifestly impossible. In a group of psychotic patients showing so many varieties of behaviour, it seems to me impossible to carry out any matching except with regard to chronological

age, sex, or test age, which are meagre evidence of equivalence. Even if the experimental and control groups were matched in weight, height, sex, education, scores in specific tests, etc., I should not be convinced that they were in any real sense equivalent. Only by having very large groups—probably of 200 or more—could one be at all sure that other individual differences would be at all evenly distributed. In small groups of 25 or 30 patients, the use of controls to counterbalance the effects of ageing over a 9-year period may well be irrelevant.

However, this methodological criticism does not to any degree detract from the significance of Smith's findings that marked decrements in Maze Test performance occurred, amounting to 2.52 years of test age or 18 points of test quotient, after the lapse of almost 9 years after operation. But it should be noted that Smith apparently used the original Maze for this repeated examination instead of the practice-controlled Extension Series. Data obtained by the writer on a non-hospital population (criminals) show that there is a gain of about 2 years after an interval of 4½ years, as a result of practice. The decrement would undoubtedly have been considerably greater had the practice-free form of the test been used with Smith's subjects.

If the seventeen patients who suffered the more severe operation of superior frontal ablation are considered as a separate group, the loss in Maze scores affected 16, or 94 % of cases, to the extent of a mean decline (below their pre-operative level) of 3.88 years, or 28 points of test quotient. Smith also studied his cases in two groups according to age, the first or older group averaging 50 years, the second 34 years. The older patients, both superior and orbital operates, showed a decline, but younger orbital cases did not lose in post-operative score.

In a letter summarizing this study which Dr Smith sent to the writer (1958*b*), he makes the following statement:

'As Goldstein has pointed out frequently, gross or composite scores as I.Q.'s are of little value and often misleading. The unique nature

of the Porteus Maze test and its greater sensitivity in discriminating the operates with superior topectomies from all other operates and controls is shown by comparison with the 11 subtests of the Wechsler-Bellevue, the Capps Homograph and Weigl Sorting test. It also discriminated older subjects with operations from non-operates much more reliably than the remaining measures.'

Thus, though there has been a marked decline in the incidence of psychosurgery cases in U.S.A., making it impossible to measure the actual deficits in Maze performance by the Extension series, Smith's demonstration that the deficits are not transient but permanent, and that they increase with age, seems to be conclusive. In his paper read to the Eastern Psychological Association, Smith said: 'In contrast to the conclusions of the original study and the two related Columbia-Greystone projects of "no permanent deficits in intellectual functions" and a "drop and rise" pattern resulting from psychosurgery, re-examination eight years later showed marked and statistically significant decrements for operates in the Porteus Maze and seven other measures.' These other tests were Arithmetic, Picture Arrangement, Block Design, Object Assembly, Digit Symbol, Digit Span, and Capps Homograph.

He further cites neurological studies by Yakolev, von Monakow, Meyer and others who reported confirmatory evidence 'that more marked and rapid degeneration of functionally and/or anatomically linked areas follows damage to the more caudal superior region, as contrasted with little subsequent degeneration resulting from lesions in the anterior portions of the frontal lobes'. He also quotes Meyer to the effect that older cerebra exhibit earlier and more rapid spread of degenerative changes with leucotomy lesions. The cumulative evidence from such sources seems to indicate that the Maze has superior sensitivity to frontal brain damage.

The relative insensitivity of most other tests, including particularly the verbal portions of the Binet and Wechsler-Bellevue scales, is

extraordinary. The fact that they do not reflect adequately serious damage to about one-third of the brain certainly raises the question of their value for mental diagnosis. Volk & Bruell (1956) have reported recently on a man who suffered a right hemispherectomy, including most of the globus pallidus, putamen, caudate nucleus, and a considerable portion of the thalamus. Apparently this patient needed only half a brain to obtain a verbal I.Q. of 125, which was his post-operative rating. He was tested in March of 1955 after his discharge from the hospital 71 days after operation. In a summarized report by Bruell & Albee (1957), these authors comment on the fact that the patient's language did not reveal any neurological impairment. His vocabulary definitions were superior. In the similarities subtest of the Wechsler-Bellevue, he showed above-average ability in abstract thinking. His Rorschach responses were well organized and his sense of humour seemed excellent. Immediate and recent memory were well retained as indicated by his information and vocabulary scores.

A blind analysis of the Rorschach performance, contributed by an expert, stated that his level of functioning was most mediocre. 'He seemed filled with anxiety-ridden fantasies of being torn, mutilated and/or deformed.' It would seem to the writer that these ideas were far from fantastical considering the circumstances. The expert also noted that the patient was 'schizophrenic-like in his handling of relationships and in his methods of coping with the complexities of reality. Paranoid features likewise are dominant in the record. Finally, there are many indications of organic impairment that should be investigated.' Considering the circumstances, it would seem such an analysis is either obvious or woefully inadequate. A similar blind analysis of the Draw-a-Person test commented on the patient's neatness, careful organization, consistency of techniques, and suggestions of schizophrenia with suspicions of an organic basis. It seems a great pity that the evidence for the sensitivity of the Maze to brain damage did not suggest

to the investigators its inclusion in their test battery. In any case, it would appear that the integrity of a far larger portion of the brain is involved in the mediation of performance test functions than verbal abilities. This fact, however, does not seem to be reflected in diagnostic practices of clinical psychologists.

That other portions of the brain, particularly the temporal lobes, are also necessary for Porteus Maze test performance seems well attested. Brown, French, Ogle and Jahnsen (1956) report a decrease in Porteus Test quotient of 24 points after temporal lobectomy, and they comment on its magnitude and consistency. In contrast to this finding, they state that 'neither pre- nor post-operatively did these patients consistently give test evidence of the kinds of intellectual deficit traditionally associated with organic brain damage. Porteus Maze Test results suggest that temporal lobectomy may result in impaired vigilance and foresight and that this impairment may be larger than the similar impairment which has been found to follow frontal lobe psychosurgery.'

In 1957 a study of considerable current importance was published. It concerned the effect of chlorpromazine (largactil) when administered to chronic psychotics as a daily dosage of 300 mg. for a period of 4 months. Much to the surprise of the investigator, a distinct loss was found in post-medication Maze scores amounting to 2.08 years of test age. The method adopted was a comparison before and after 4 months' drug administration, of standard or original Vineland Revision Maze performance with the Extension series. This was a greater decrement than that found for lobotomy patients, but in the latter instance the Vineland Revision had been repeated, with practice effects.

The same year, Porteus and Barclay followed up this study with a further investigation involving a larger number of subjects ($N=35$) and controls ($N=25$). In this group the post-medication loss amounted to 1.89 years, while the control group was only 0.1 year. Two groups of 20 control and experimental patients,

each matched exactly by original or standard Maze scores, showed a mean loss of 2.2 years for the drug or experimental group, while the controls had an average gain of 0.2 year. It should be stated, in view of our previous discussion, that the use of controls was a conventional procedure.

It had been noticed by Porteus & Peters (1947) that some lobotomy patients, who did not exhibit an immediate post-operative Maze deficit, did so in a later examination. The percentage showing a decline in scores rose from 68 to 82 %, when all who fell below their pre-operative level at the first or a later post-operative testing were considered. This accords with Smith's experience of a gradient in deficits. A further study involving 20 mental defectives, who were given 100 mg. daily for 6 weeks, was also carried out. They showed an average loss of one year in Maze score. A note on this last investigation will be published shortly.

As Landis has suggested, one of the main factors affecting success in the Maze seems to be vigilance or, in my own terms, mental alertness. There is evidence that the reticular formation in the brain stem has as one of its functions sustaining vigilance as a self-preservative function. If the more direct nerve paths from the reticular system pass upward to the superior cortex of the frontal lobes, that could account for the Maze impairment that follows damage to that frontal region. Landis and others have remarked that the reactions of a frontal lobe patient resemble those of persons who are drugged or sleepy.

It has also been suggested that the locus of operation of chlorpromazine is also the reticular formation, and if so, that would strengthen the assumption that the Maze impairment following prolonged use of the drug is also related to the functions of the reticular frontal complex. As against the assumption of equipotentiality of the brain, the writer has put forward a theory of 'centroid neurobiotaxis,' which is supported by Smith's findings.

If this theory is further supported by additional evidence, it would strengthen the claim

that planfulness is part of the earliest, most fundamental mental equipment essential to survival, and that its specific examination by the Maze, or a similar test, is a necessary part of mental diagnosis. This is the position maintained by the writer for 40 years, and it would be interesting to have it upheld by such recent work.

A doctoral dissertation presented by Aristotle Anthony at Teachers' College, Columbia University, investigated the effect of stress on Qualitative Maze performance (Q-score), the subjects being a control group of public school pupils, contrasted with an experimental group who, because of bad conduct and poor academic performance, had been removed from public school and sent to the Devereux Schools for special education and psychotherapy. Conditions of stress were arbitrarily imposed in a second application of the test by implied criticism of the first response. The need for quicker performance was emphasized, and in the second testing additional stress was occasioned by a buzzer sounded at 'choice' points in the Maze, with a double buzz to signify that so much time was being wasted that a zero score was likely.

Four hypotheses were tested: firstly, that Q-scores under stress would be greater—supported beyond the 0.001 level of confidence; secondly, that neurotics would have greater Q-scores than non-neurotics—supported beyond the 0.001 level; thirdly, that the number of mazes required would be greater under stress—supported beyond the 0.001 level; and fourthly, that difference between stress and non-stress scores would be greater for neurotics—supported beyond the 0.05 level. Other similar hypotheses pointing to differences between the two groups were also tested and confirmed. As a whole, this study supports previous findings of the value of the Q-score in differentiating delinquent and non-delinquent personalities, as indicated by their different approaches to a task involving what M. Clement has called '*l'intelligence utilisable*'.

But another current investigation being carried on in Hawaii as a subsidiary approach

to the problem of the duration of chlorpromazine Maze deficits, a project supported by a grant from the National Institute of Mental Health, promises to have many very important educational and therapeutic implications.

This was the discovery that by taking account of certain projective-expressive aspects, chiefly the style of beginning and ending three tests of the Extension series, it was possible to match without names or other identifying data two performances by the same individual. This can be done successfully in about 90 % of cases, both delinquent and non-delinquent, and in a slightly greater per cent (about 95 %) with adult criminals.

The procedure consists of placing the first records for years VII, X and XI on a table with a strip of cardboard stapled over the name written across the top. The second performances, with the names similarly hidden, are then placed in random order on one side. Paying particular attention to where the test is begun in relation to the arrow marking the starting point, the figure or shape made by the first couple of strokes in threading the maze design and the appearance or form of the endings, the observer tries to match correctly each individual's performance. In most cases these points of comparison are enough, but in difficult cases the pressure on the pencil, general irregularities of line, the occurrence of right-angled turns and the general *gestalt* may be also taken into account. The percentages of correct matchings quoted above refer to procedures when the three Extension tests (VII, X and XI) are given immediately after the application of the whole series. After an interval of a month between the first performance and the second, the percentage of correct matchings with delinquents drops to 70, but this is about 9 times the percentage when duplicate tests are put together at random.

The executive similarities are termed projective as well as expressive because their appearance seems to be unconscious on the part of the subject. If he begins the test at exactly the same point in relation to the arrow, or makes the same kind of ending, he is

seemingly unaware of the fact. With the focus of attention directed towards the task of finding his way through the maze, details of execution seem to be subliminal as regards consciousness, and this allows persistent or characteristic styles of responses to exhibit themselves.

There are, however, extreme differences in self-consistency shown by individuals, and the exploration of the meaning of these differences should open up new fields of research. Apparently, there are sex differences as well as differences between delinquents and non-delinquents, and the writer, assisted by Mr John Barclay, has worked out a system for quantifying degrees of similarity or self-consistency. The remarkable thing is that all of us are such slaves to habit that we repeat ourselves, not only in commonly repeated activities, but even in a maze performance given only once previously.

The educational implications are related to the degree to which this repetition of details of performance approaches compulsiveness and whether these compulsions, when present, are good or bad. Furthermore, are they the products of education and training?

It is the writer's belief that the individual rather than the group differences are of greatest importance, but their social or intellectual implications await extended study. As regards the Maze, matching success provides conclusive evidence that qualitative reactions of an individual show a reliability or self-consistency that in turn confirms the extreme reliability of the test.

It is confidently expected that the successful working out of a system of quantifying resemblances will enable the psychologist to correlate this with other abilities or aspects of behaviour in a manner not possible with unquantified indices of personality such as those provided by the Rorschach.

Provided that these Maze projective-expressive features form an important trait complex in personality, it should be possible to discover by comparison of scores what changes occur as the result of pharmacology, brain operations, and different forms of psychotherapy. Furthermore, the method should demonstrate how wide or how narrow is the distance that separates the criminal from the non-criminal, the abnormal of various types from the normal.

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ANOREXIA NERVOSA: A CONTRIBUTION TO ITS PSYCHOGENESIS

BY LINTON GRIMSHAW

Most authorities consider anorexia nervosa to be primarily a psychogenic disorder (Ryle, 1936; Sheldon, 1937; Richardson, 1939; Farquharson & Hyland, 1938; McCullagh & Tupper, 1940; Nemiah, 1950). Kay & Leigh (1954), in their review of thirty-eight cases, reported various precipitating factors, notably disturbance of interpersonal relations, voluntary dieting and physical illness. Attempts to demonstrate a specific psychological cause have been made by detailed and lengthy studies of individual cases (Rahman, Richardson & Ripley, 1939; Waller, Kaufman & Deutsch, 1940; Moulton, 1942; Lorand, 1943). These writers believe that unconscious phantasies of oral impregnation, repressed in early life and revived under the stress of puberty, are responsible. Often, however, it is difficult to see a direct relationship between such phantasies and the symptoms. Although they are fertile sources of psychopathology, such studies are few since a good relationship with the patient is hard to secure.

The case which follows is presented, for despite its difficult course the patient remained fundamentally co-operative throughout. The material brought to light was of psychological significance, and its discovery was followed by clinical improvement.

The patient was a 17-year-old single girl. She was suffering from anorexia nervosa, and showed loss of appetite and weight with amenorrhoea in the absence of organic disease. A year before admission, in the summer of 1956, she became concerned about her performance in the final school examination, particularly the effect it would have on her father if she failed. In October of that year her worries increased for, although her examination results were satisfactory, she had in deference to her father begun to prepare for a career in teaching. This was against her own

wish to take up nursing. In fact, the mere thought of teaching gave her 'spasms of fear in the stomach'. Her father was recovering from a coronary thrombosis and she felt any worry might even threaten his life. Because of her great fear of him and concern for his health, she avoided an open disagreement about her career. Her hatred of teaching and an intense resentment were continually present, combined with strong guilt feelings that she could secretly wish to defy her father.

In this setting she noted her appetite steadily deteriorating. In October her menses stopped. At Christmas her weight loss was marked. All her social interests disappeared and her school work was left undone. She felt conscience-stricken about disappointing her parents, again particularly her father who was inordinately ambitious for his children. Indecision about her career was still present. Whilst in bed she would ruminate in a compulsive fashion about religious and philosophical topics. Finally, her weight reached 6 stone, a loss of 2½ stone, and her physical condition necessitated admission to a general medical unit. No organic disease was found despite extensive physical investigations, including X-rays of skull and chest, blood chemistry and metabolic investigations. The urinary 17-ketosteroid estimation was within normal limits (4.5 mg. in 24 hr.).

The patient had measles and mumps as a child and acute appendicitis with an appendectomy in May 1956. There was no history of previous mental or nervous illness.

She was the third of five children. All the siblings were in good health and showed no psychological disturbance. Her mother, aged 57, was an anxious woman, pre-occupied with the eating habits of her children. The illness had led to repeated battles between the patient and her mother at table. Her father, aged 58,

was an over-conscientious and ambitious man with high standards for himself and his family. Puritanical and authoritarian, he would closely regulate the behaviour of his children. He suffered from hypertension and had had a heart attack in 1956.

The patient's birth was normal but she and her mother were evacuated immediately afterwards to Wales because of the outbreak of war. There was no separation from the mother in childhood. She was regarded as an over-active child and was a nail-biter. No feeding difficulties were reported. She attended a grammar school and was an able pupil. The menarche was reached at the age of 13; until the illness began her periods were normal, apart from first-day pain, occurring every 28 days.

Her personality was predominantly obsessional—orderly, perfectionistic, with evidence of insecurity. No hysterical traits were noticed.

On examination she was greatly underweight. Pubic and axillary hair was present. Her body was covered with fine downy hair but no other physical abnormality was noted. There was no obvious mood change but she appeared over-active, especially in the kitchen where she would help prepare food for the patients, disdaining a crumb for herself.

Psychological testing (carried out by Mrs P. Bunn) showed an I.Q. (Wechsler-Bellevue) of 123. Projective tests indicated deep-seated conflict over sexual matters. This will be reported in greater detail in a further communication.

TREATMENT

It was decided to treat the patient by psychotherapy. At first the intake of food was ignored and no comment was made if the food remained untouched. The first interviews were concerned with occupational matters. The patient was encouraged to talk freely about her choice of career and take positive action about it. She began to help Sister in the wards, ate more and gained a little weight. Sexual topics were then raised. The patient became

very much on guard and no great progress was made.

The weeks passed but she made no definite decision about a nursing career despite her father's agreement to it. She became bored and stopped helping Sister. Her appetite and weight again began to suffer. Finally when it was suggested to her that she did not really want to nurse, she agreed and said she felt happier for having admitted it.

She began to talk more openly about her father. Insight was gained into his behaviour which she criticized more and more. She mentioned how he controlled the activities of his children by insult and, at times, injury, if they did not comply with his orders. She described how the children formed conspiracies to hide acts of disobedience and how she always felt guilty afterwards. Throughout, her criticisms were mixed with pity for his ill-health and his hard upbringing which she said was responsible for his attitude towards his own family. Despite all this free ventilation of difficulties no improvement in her condition occurred.

She began to report a number of dreams. The first important dream was of being poisoned in a foreign country by a man and a woman. The man was in a white coat. She said this man resembled a male nurse in the hospital who reminded her of her father. The man was offering her a white gelatinous liquid in a medicine glass. He seemed to derive pleasure from offering her the drink. The patient thought that if she drank it she would die; if it entered her mouth she would have to spit it out. With this dream the patient associated events which took place when she was 5 years old. At this age after her bath, her father would give her milk of magnesia which she hated and would secretly spit out afterwards. On narrating this dream the patient noted a very transient improvement of appetite.

About this time the patient was given modified insulin treatment. On one occasion whilst mildly hypoglycaemic she performed frankly sexual movements and became very excited. She shouted at the doctor, saying she hated

him because he resembled her father whom she hated. When asked why, she replied that he had made her go off her food. Then she remarked that she was afraid of something, something to do with life, and that she was trying to forget something she could not explain.

Two important dreams were subsequently reported. In the first dream the patient was in the country: 'I am running away from something like a symbol, like justice, life, freedom.' She felt afraid in the dream, which ended with her tripping into a well of treacle. After recounting it she discussed the 'symbol', associating it with life and food from which she was running away.

The other dream which she recounted had recurred at various times throughout her life. The patient was seated behind a large white blancmange which she had to eat her way through before she could reach a door on the other side and gain 'freedom'.

The patient then began to talk of a sexual assault suffered at a party in 1955, 12 months before her illness began. She was pushed on to the stairs. The boy tried to induce her to go into the bedroom with him. She refused. He masturbated and when she escaped downstairs she noticed semen on her dress where his hand had held her. She felt disgusted and sick. She was attracted yet repelled and admitted to feeling sexual desire. She did not meet him again till 12 months later at a dance in the summer of 1956. Again attracted and repelled, she thought how he looked like an ape. She felt sick and remembered not wanting her tea and biscuits during the interval.

When recounting this incident, she recalled that her father had once similarly pushed her down. He had noticed she was wearing lipstick for the first time. He was angry, calling her a 'cheap woman'.

Shortly afterwards, she reported another dream. She was with her family, another girl and an ape. The girl was in love with the ape. Her mother said that the girl was guilty for being in love but her father said the girl was blameless. In discussion afterwards, the patient identified herself with the girl and

commented on the unexpected attitude of her father.

The patient gradually began to develop great hostility to the staff and to the other patients. She swore at everybody and was totally negativistic. She lay in bed, insulting everybody who disturbed her. Despite this, a thread of communication was maintained with the physician who passively accepted her aggressiveness. Her rage was combined with severe self-reproach, depression and despair. Whilst at home for a week end her venom was directed chiefly at her father whom she bitterly criticized—a thing she was normally afraid to do. Because of the depression, two electrical convulsions were induced but with no response. This treatment was abandoned since her physical condition was now very poor. She appeared to be entering a state of hibernation: inert, with subnormal temperature and bradycardia. At this point artificial feeding by fortified milk drip was introduced.

On discussing how she felt about eating, the patient said she felt compelled to spit food out of her mouth. She was secretly throwing food down the toilet or sink. 'Food and I don't click,' she said. 'I feel as though I can't breathe when I have food in my mouth. Also when food is there it doesn't seem to taste of anything and I hate tasteless food. I don't want food. It doesn't mean anything to me. I hate especially white foods—I always have. All food is better if tasty—If I do take food I must use a lot of salt and sauce with it'.

The dream about the blancmange recurred. She remembered being given blancmange as a child which she used to spit out. Sleep now became difficult: despite sedation she was unable to go to sleep until the early hours of the morning. Although artificially fed, her weight was still under 6 stone and she admitted at this point to disconnecting the drip and pouring milk down the sink. However, she began to reveal important psychological material. On discussing again the subject of food she said that it felt like something in her mouth which should not be there. On being asked if this was anything to do with kissing, she agreed,

saying it reminded her of being kissed by her boy friend who put his tongue into her mouth. This disgusted her because it was wet with saliva. She repeated her preference for strongly tasting food since then she knew it was food. What was intolerable was insipid food for then she was uncertain; it might be something else which should not be in her mouth. She hated white food since it reminded her of saliva.

The blancmange dream recurred. 'I hate the blancmange—It's tasteless,' the patient said afterwards. Later she said she was trying to recover a memory of something unpleasant which happened to her as a child. Persistent attempts to trace this at night were in fact the cause of her insomnia. At this stage, she could only remember grass and water and that she was about 6 years old.

On talking freely, further details of the memory were recovered. She remembered that the grass and water were actually associated with a canal bank near home. She was playing with another girl. Her parents had forbidden her to go there. There was another person present whom for the time being she was unable to identify. On one occasion this memory had become mixed up with a dream that followed. In the dream, the other person was the window-cleaner at home. The window-cleaner resembled a picture of a father in a children's story book. He was showing the patient religious pictures with an underlying sexual motive. He touched her breasts and genitals and then said 'Open your mouth and close your eyes and see what God sends you'. At this point, the dream became confused with a picture of the washing machine at home and 'my mother giving me three meals a day'.

Two days later, she said she could now give the memory complete. 'I was playing with a girl friend near mounds of earth on the canal bank. I shouldn't have been there—my parents had forbidden it. We played hide and seek and I hid behind a mound. She could not find me and wandered away. I had catarrh badly and was breathing through my mouth. A pale faced young man approached me and

asked why I was breathing like that. He said, "Open your mouth and close your eyes and I will give you a sweet". This the patient did and the man then put his penis into her mouth. She immediately felt unable to breathe and the sense of something tasteless in her mouth. It also felt wet and sticky and she had to spit the semen out. The man then ran away. Afterwards for a time, everything she ate seemed tasteless.

After recounting this incident, the patient said spontaneously that it coincided with the period her father was pressing milk of magnesia on her which she would spit out. The illness began in the summer of 1956 at the time the act of tongue-kissing mentioned above occurred. This happened at the same spot, namely near the mounds on the canal bank when she was being taken home. She remarked that she had always been afraid of the canal but was unaware of the reason. Previously she could only recall fragments of the event.

Following this recollection, the patient's appetite immediately began to improve. She soon began to worry if her milk feed was overlooked and called for more of it to be given. Her bodily appearance changed; her hair became glossy and there was an increased fullness of her breasts. The sleep disturbance stopped and no more dreams were reported. Her personality seemed to have matured: she appeared more self-composed and confident. Finally she began to take normal meals, made preparations for training in secretarial work and was discharged home 1 month later. Before her illness began she weighed 8 st. 4 lb., on admission 6 st. falling to 5 st. 10 lb., and on discharge 7 st. 2 lb. At periods during the illness she was given various physical treatments: modified insulin, Chlorpromazine, electroconvulsive-therapy, an anabolic agent—Norethandrolone ('Nilevar'), and finally, in view of her serious condition, milk feeds through a Ryle's tube. None produced improvement, and even forced feeding failed during the first few weeks to produce any material change since when not supervised the patient poured the milk down the sink.

On discharge from hospital, the patient left the district to work in the London area. She reported by letter 3 months later that she was well, her appetite was fair and she was maintaining her weight, but her periods had not returned.

DISCUSSION

The natural history of the illness suggests that the early incident of fellatio, occurring at an age when her father was forcing medicine on her, had been repressed by the patient. In the following years, it had declared itself only indirectly in dreams, and unexplained aversions to certain foods and the canal bank. The tongue-kissing incident was critical for the reactivation of the repressed but forbidden oral sexual wishes, and there found symbolic expression in the form of symptoms. Food was invested with a meaning derived from the early traumatic event. Equated with the penis and semen, it was consequently intolerable and rejected.

All these experiences had occurred in a setting of difficulties with her father. The early event was linked in the patient's mind with her father forcing medicine on her; the later one, she felt, was associated with his pressing an unwanted career. Her attitude to him was coloured by guilt for her oedipal wishes, and that she should wish to defy him when his health, his very life even, was threatened. The sexual incident at the party appears to have been a contributory factor. It was her first adolescent sexual adventure, and reactivated her mixed feelings towards her father.

During the illness considerable friction occurred between the patient and her mother. It was, however, related chiefly to feeding, and the mother's attitude was fully understandable in terms of anxiety evoked by food refusal. It is, of course, probable that conflicts with her mother on both oedipal and oral levels, stressed by other writers, were present and would have been demonstrated more clearly with further study.

The symptom-complex of anorexia nervosa in this particular case seems to be a hysterical conversion syndrome. It was an attempt to relieve a repressed emotional conflict by symbolic somatic substitutes, which had, as Freud insisted, an ultimate relationship to ungratified sexual tension. There appears to be sufficient evidence for believing the patient to have been engaged in an Oedipal struggle at the time of the sexual traumas which became invested with phantasies of oral impregnation—in the final denouement, by her father.

It is believed that the revival of the early conflict, and the discharge of associated emotion with psychotherapy, produced improvement. It may be objected that any of the physical treatments, including forced feeding, may have been ultimately responsible. No change was noted, however, in the patient's condition following any of these methods; there was, in fact, a slow but steady deterioration until a revival and understanding of the psychological conflict had been achieved.

SUMMARY

A case of anorexia nervosa is presented showing features of aetiological importance. The revival, during adolescence, of a repressed early childhood incident of fellatio in an atmosphere of great difficulties with the patient's father is put forward as the chief causative factor. With the full recollection of this early sexual experience during psychotherapy, improvement appeared to follow. Reasons for regarding the symptom-complex as hysterical and its relation to phantasies of oral impregnation are mentioned.

ACKNOWLEDGEMENTS

I wish to thank Prof. Anderson, under whose care the patient was admitted, for permission to publish this case, and for valuable advice and criticism. I am also grateful to Dr J. Hoenig for helpful encouragement.

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THE ASSESSMENT OF ANXIETY STATES BY RATING

BY MAX HAMILTON*

In the last decade many scales have been devised for the assessment of psychiatric symptoms. Most have been designed for use with patients in mental hospitals and have therefore concentrated chiefly on behaviour in the ward and in hospital activities. Not many of the items are concerned with symptoms, and these are chiefly those of schizophrenia and the depressive psychoses. Even less attention is paid to neurotic symptoms, especially anxiety states, despite the fact that the scales are intended generally to cover the full range of psychiatric syndromes. These scales have been designed to enable the research worker to obtain a quantified measure of the patient's clinical status, e.g. for use in clinical trials of treatment. In them, the separate items are summed in groups and a set of scores or 'profile' is obtained for each patient. This 'profile' is often used as a diagnostic aid, although this is not the primary purpose of the scale. Users are generally warned not to use the scale for making a diagnosis.

In practice, these scales have two other functions of great importance. The first is that the investigator can describe precisely certain characteristics of his group of patients using the mean score and standard deviation. The description and definition of the population from which a sample is drawn is of fundamental importance and is one of the difficult problems that faces research in psychiatry. For this purpose diagnostic categories are notoriously unreliable and rating scales are invaluable. The second function is that they help to define syndromes and subsyndromes,

and in a manner which permits of reproduction in another enquiry.

The present scale was designed along different lines. It is intended for use with patients already diagnosed as suffering from neurotic anxiety states, not for assessing anxiety in patients suffering from other disorders. Anxiety in greater or lesser degree is found in agitated depression and obsessional states particularly, and also in such states as organic dementia, hysteria and schizophrenia, but it must be clearly emphasized that the scale is not intended to cope with these conditions.

The usual methods for scale design were used. A series of symptoms were assembled which were considered to cover the condition adequately. These were then grouped together according to their nature, or where clinical experiences indicated that they were associated. It was decided that for practical purposes twelve groupings were sufficient. Together with the patient's behaviour at interview, these formed the thirteen variables of the scale. They are: anxious mood (a continued state of apprehension), tension (including irritability), fears (of specific or phobic type), insomnia, cognitive changes (difficulty in concentration and forgetfulness), depression, somatic symptoms of a general type, cardiovascular, respiratory, gastro-intestinal, genito-urinary, and general autonomic symptoms, the latter consisting chiefly of headaches and sweating. Each of the variables was defined in a series of brief statements, headed by the name of the variable, printed on a sheet which faced the interviewer during the interview with the patient (see Appendix 1).

Assessments were made on a five-point scale (see Appendix 2). In practice, the last grade is very rarely used for out-patients, and serves more as a marker, a method of delimiting

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the range, rather than as a grade of practical use. In order to determine the reliability of the scale the patients were seen by two interviewers simultaneously. The principal interviewer conducted the interview and endeavoured to obtain information regarding the patient's symptoms. The second interviewer made his ratings independently of the first and could add his own questions if he thought he had not had sufficient information.

Table 1. *Correlations and t tests between raters*

	Raters			
	I	II	III	
I <i>t</i> test	—	0.30	0.54	First inter- view
Correlation	—	0.93	0.39	
No. of subjects	—	8	8	
II <i>t</i> test	0.07	—	0.63	
Correlation	0.83	—	0.91	
No. of subjects	8	—	10	
III <i>t</i> test	0.64	1.30	—	Second interview
Correlation	0.95	0.93	—	
No. of subjects	8	10	—	

The initial testing of the scale involved three psychiatrists. Preliminary discussions eliminated many of the difficulties in the first version of the definitions of the variables. The rating scale was then tried on a number of patients and the discrepancies and agreements between psychiatrists carefully considered in detail, in an endeavour to eliminate difficulties. The scale was then tried on a group of patients and this paper is concerned with the results. An identical procedure was followed throughout. Each patient was assessed by two raters and the results recorded. Afterwards the results were compared and any discrepancies noted and discussed. Nevertheless, once a rating had been made it was not altered. The measure of reliability was based

on the sum of crude scores for each patient. Product-moment correlations were calculated between each pair of physicians, and since the patients were interviewed on two occasions for purposes of a drug trial, two such correlations between each pair of physicians is available. The results are to be seen in Table 1. The weighted mean of these correlations, using the *z* transformation, is 0.89. This is remarkably high and illustrates the reliability of psychiatric assessments under suitable conditions. Since the reliability coefficient does not give information on the bias of raters towards high or low scores, *t* tests were calculated between pairs of raters in the same way (see Table 1). The weighted mean of these *t* tests is 0.61 and shows that very little bias is to be found.

The relations between the variables were then examined. Product-moment correlations were calculated between the variables and the resultant matrix factor-analysed by the method of Simple Summation (the matrix of correlations is available on request). Communalities were estimated by five iterations of the process. This is very easily done using the shortened method of Burt (1949). A general and one bipolar factor were extracted. The general factor is clearly a general factor of anxiety and the bipolar divides the symptoms into two groups: The first contains psychic symptoms consisting of tension, fears, insomnia, anxiety, intellectual (cognitive) changes, depression, and behaviour at interview. This was contrasted with a group of somatic symptoms consisting of gastro-intestinal, genito-urinary, respiratory, cardiovascular, somatic general and autonomic symptoms (Table 2).

When the factor saturations are plotted it may be seen that the vectors lie almost completely within a right angle. In other words it is possible to rotate the saturations to give two orthogonal group factors. The variance of the general factor constitutes 27 %, of the bipolar 18 %, giving a total of 45 % of the total variance. This total was probably reduced by selection.

Table 2. *Saturations for centroid and rotated factors*

	G	BP	I	II
Tension	0.60	0.26	0.36	0.54
Fears	0.29	0.37	0.04	0.46
Insomnia	0.79	0.32	0.48	0.70
Anxious mood	0.43	0.75	-0.06	0.86
Cognitive changes	0.56	0.07	0.42	0.37
Depression	0.38	0.52	0.02	0.64
Behaviour	0.37	0.22	0.18	0.39
Gastro-intestinal symptoms	0.41	0.00	0.34	0.22
Genito-urinary symptoms	0.43	-0.34	0.55	-0.05
Respiratory symptoms	0.31	-0.54	0.56	-0.27
Cardiovascular symptoms	0.34	-0.62	0.62	-0.33
Somatic (general) symptoms	0.48	-0.31	0.57	0.01
Autonomic symptoms	0.56	-0.10	0.52	23
Communality	2.93	2.09	—	—
Communality as percentage	23	16	—	—

DISCUSSION

This particular matrix of correlations can be resolved either into a general factor of anxiety and a bipolar factor of psychic versus somatic symptoms, or alternatively, into two orthogonal group factors of 'psychic anxiety' and 'somatic anxiety'. Since both factorizations give orthogonal factors, there is no advantage in one over the other. On general grounds, we know that had there been less selection of subjects, so that they extended through the full range from those with trivial symptoms to those severely ill, then in the centroid analysis, the general factor would have had a greater variance, the bipolar factor still being orthogonal to it. In the group factor analysis, the two group factors would have been positively correlated, this implying a general second order factor. The British school of factorists, following Burt, emphasize the value of orthogonality. The American school, following Thurstone, emphasize the value of being able

to identify the same factors regardless of the problems introduced by selection. In this particular case, the group factor analysis has the advantage of orthogonality as well.

Despite the apparent advantage of the group factor approach over the general factor approach, it must not be forgotten that mathematically, the two have an equivalence, since the one can be converted into the other by a simple transformation, in this case, the simplest of all, an orthogonal rotation. No new information can appear from such a transformation. (In fact, factor analysis, except for the method of principal components with full variance, actually loses information. Its great advantage is that it makes information clearer and more comprehensible.) The choice between general and group factor analysis must depend on other considerations.

In this case, the selection of patients is based on the fact that they all suffer from anxiety neurosis, and this condition shows itself as a general factor, i.e. a dimension to which all the variables are positively correlated, or on which they all have positive non-zero projections. It may be that, in other circumstances, the division into group factors may be preferred. For example, the response to treatment, or the effects of some drug, may show as a change in one or other of the group factors. Even if this should be so, it would only mean that whereas for such a situation, the group factor is appropriate, for the present situation, i.e. for diagnosis, the general factor is the appropriate one.

It is interesting to compare this rating scale with the factor analysis of the Taylor scale by O'Connor, Lorr & Stafford (1956). Although the present scale is concerned with general symptoms, whereas the Taylor scale deals with specific statements, the two factors A and B correspond roughly with the present general and bipolar factors. Factors A and B correlate 0.068, so they too are orthogonal.

Both the Taylor scale and the scales of Dixon, de Monchaux & Sandler (1957*a, b*) differ from the present one in that they are concerned with the content of the patient's

symptoms, rather than the form. This is also true of the Taylor scale. Although in the course of treatment the specific nature of a patient's fears and anxieties may change, it does so much less readily than the intensity. The assessment of both these kinds of changes is of practical and theoretical importance, and therefore the two kinds of scale are complementary.

The present scale obviously invites comparison with that designed by Buss, Wiener, Durkee & Baer (1955). It is important to recognize the difference between the two. The present scale is designed for the rating of anxiety neurosis as a syndrome, not for the rating of anxiety. Until the contrary is proved, it must be regarded as invalid for the rating of anxiety in any other setting. This limits the range of usefulness of the scale but, within these limits, patients can be compared meaningfully. It places great emphasis on the patient's subjective state. (This follows from the medical bias of the author, for in treatment the patient's subjective state takes first place both as a criterion of illness, which brings the patient for treatment, and as a criterion of improvement.) The various symptoms are rated separately, the somatic ones being given equal place with the psychic. This is because in outpatient practice patients place great emphasis on somatic symptoms, and a large number go first to the general medical departments for investigation of these. The scale of Buss *et al.* was used for rating anxiety on all types of patient except those suffering from cerebral damage. It therefore has a wider range of application. This is counter-balanced by the fact that the comparison of scores for anxiety, e.g. schizophrenia, depression and anxiety neuroses, has no clear meaning. It assembles symptoms into fewer groups. It gives less weight to somatic symptoms, or alternatively, gives more weight to psychic symptoms. Both scales group many single items under a limited number of headings, and it would be clearly desirable to investigate the appropriateness and usefulness of this procedure. Both show high reliability in use. I do not intend to

suggest that either scale is better than the other. Only practical use will determine which is the more useful, and it is to be hoped that both will be superseded by something better.

The scale can by no means be considered to be in its final state. Ideally, each of the items listed under the heading of a variable should be handled separately for purposes of full item analysis. The sheer labour of doing this in a rating scale (as opposed to a questionnaire) will delay this for a long time. Some of the variables are obviously a rag-bag of oddments and need further investigation. Further work is being done in which the general somatic symptoms are separated into two variables: muscular and sensory.

Experience has shown that grade 2 can be split up into two grades without increasing the difficulty of rating. In practice, grade 4 is almost never used because the rater is reluctant to give the maximum score to subjects who could obviously be much worse. An additional grade would probably be rarely or never used, but would encourage the rater to subdivide grade 3, shifting some of his ratings to the higher grade.

SUMMARY

A rating scale for the symptoms of anxiety neurosis has been prepared as an aid to the quantification of symptoms. It was used on thirty-five patients by three physicians working in pairs. The reliability of the scale, as shown by correlations and *t* tests between raters, is high. The correlations between variables can be factorized into a general factor of anxiety and a bipolar factor contrasting psychic with somatic symptoms; or into two orthogonal group factors of 'psychic' and 'somatic' anxiety.

ACKNOWLEDGEMENTS

I would like to thank Prof. Hargreaves for permission to publish this paper, and particularly for his advice and guidance in the design of the rating scale. I have to thank him and Dr Roberts for taking part in the ratings of patients. The Research Fellowship is supported in part by a grant from the Mental Health Research Fund to whom thanks are due.

APPENDIX 1

*Symptoms of anxiety states**Anxious mood*

Worries
Anticipation of the worst
Apprehension (fearful anticipation)
Irritability

Tension

Feelings of tension
Fatiguability
Inability to relax
Startle response
Moved to tears easily
Trembling
Feelings of restlessness

Fears

Of Dark
Strangers
Being left alone
Large animals, etc.
Traffic
Crowds

Insomnia

Difficulty in falling asleep
Broken sleep
Unsatisfying sleep and fatigue on waking
Dreams
Nightmares
Night terrors

Intellectual (cognitive)

Difficulty in concentration
Poor memory

Depressed mood

Loss of interest
Lack of pleasure in hobbies
Depression
Early waking
Diurnal swing

General somatic (muscular)

Muscular pains and aches
Muscular stiffness
Muscular twitchings
Clonic jerks
Grinding of teeth
Unsteady voice

General somatic (sensory)

Tinnitus
Blurring of vision
Hot and cold flushes
Feelings of weakness
Pricking sensations

Cardiovascular symptoms

Tachycardia
Palpitations
Pain in chest
Throbbing of vessels
Fainting feelings
Missing beat

Respiratory symptoms

Pressure or constriction in chest
Choking feelings
Sighings
Dyspnoea

Gastro-intestinal symptoms

Difficulty in swallowing
Wind
Dyspepsia:
 pain before and after meals
 burning sensations
 fullness
 waterbrash
 nausea
 vomiting
 sinking feelings
 ‘Working’ in abdomen
Borborygmi
Looseness of bowels
Loss of weight
Constipation

Genito-urinary symptoms

Frequency of micturition
Urgency of micturition
{ Amenorrhea
 Menorrhagia
 Development of frigidity
{ Ejaculatio praecox
 Loss of erection
 Impotence

Autonomic symptoms

Dry mouth
Flushing
Pallor
Tendency to sweat
Giddiness
Tension headache
Raising of hair

Behaviour at interview (general)

Tense, not relaxed
Fidgetting: hands,
 picking fingers,
 clenching, tics,
 handkerchief
Restlessness: pacing
Tremor of hands
Furrowed brow
Strained face
Increased muscular tone
Sighing respirations
Facial pallor

Behaviour (physiological)

Swallowing
Belching
High resting pulse rate
Respiration rate over 20/min.
Brisk tendon jerks
Tremor
Dilated pupils
Exophthalmos
Sweating
Eye-lid twitching

APPENDIX 2

Date

Anxious mood

Tension

Fears

Insomnia

Intellect

Depressed mood

Somatic general (muscular
and sensory)

Cardiovascular system

Respiratory system

Gastro-intestinal system

Genito-urinary system

Autonomic system

Behaviour at interview

Grades

0 is none

1 is mild

2 is moderate

3 is severe

4 is very severe,
grossly
disabling

General comments:

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SCIENTIFIC METHODOLOGY AND 'THE DYNAMICS OF ANXIETY AND HYSTERIA'

BY H. J. EYSENCK

In their recent critique of *The Dynamics of Anxiety and Hysteria* Eysenck (1957), Storms & Sigal (1958) have made a large number of comments and criticisms. I do not intend in this brief reply to take up all the points they mention. In the first place, many of them have already been dealt with elsewhere (Eysenck, 1958*a-d*). In the second place, anyone replying to a critique of his work is faced with an awkward choice which appears to be inescapable. If he wishes to answer each point separately, his reply would become impossibly long as he would have to recapitulate what he said or did in the first place, and what the criticism was, before being able to answer it. The alternative method of answering only what would appear to be the main points easily lays him open to the charge of evasion. I have chosen the second course, namely to discuss a few major points rather thoroughly, in preference to discussing every point raised in the short and unsatisfactory manner necessitated by space requirements. In the third place, it seems to me that Storms and Sigal go wrong not so much in point of detail (although here they also make some curious mistakes and errors), but rather by having an altogether unrealistic notion of scientific methodology in general, and the question of proof in particular. Such points can best be brought out in a more general discussion.

To begin, I will take a point made early on by Storms and Sigal. They say that my statement about individual differences in excitation and inhibition being properties of the physical structures involved in making stimulus-response connexions, 'implies that measures of personality dimensions based on these differences should be relatively stable over time within the same individuals. However, no longitudinal studies of stability or shift of his

dimensions have been conducted'. It is difficult to see quite what it is that Storms and Sigal are criticizing. They have stated my hypothesis correctly and they have also stated the deduction from it in a manner which is at least not incorrect. This is one of many hundreds of deductions which can be made from my theory, and it undoubtedly deserves to be taken up and tested. That no such test has hitherto been performed is surely no criticism of the theory, which in any case was put forward only a relatively short time ago, thus making it quite impossible for any proper longitudinal studies to have been executed. What Storms and Sigal appear to imply is that no theory should be put forward until all the possible deductions from it have been tested. This, of course, is equivalent to saying that no theory should ever be put forward, as at the beginning no, or only very few deductions, have usually been verified, and as at no time will more than an infinitesimally small proportion of all possible deductions have been submitted to scrutiny. Storms and Sigal mention several other deductions which they consider to follow from my theory, and at various points take me to task for not having tested these deductions; this appears to be a rather unrealistic type of critique. In spite of the large-scale efforts that have gone into the testing of Hull's postulates, only a very small number of deductions have in fact been submitted to the experimental test. To pick out one or two of those not yet verified, and use the fact that they had not yet been tested to discredit the Hullian system would quite rightly be regarded as absurd.

Nor is this all. The history of science shows that it is often possible for a vital deduction from a scientific theory to be apparently falsified by facts for a long time, although the

theory was in fact correct. Thus it follows from Copernicus's heliocentric theory that parallax should be observable among the stars, yet for hundreds of years after his death astronomers failed to discover any evidence of parallax. The *ad hoc* hypothesis that the stars might be so far away as to make the observation of parallax with existing instruments impossible was duly laughed out of court by his opponents. Yet both theory and explanation of contrary findings turned out to be correct. Thus it is by no means essential for a scientific theory at its point of inception to predict all phenomena observable at the time; certain matters may have to be left for further study because methods for correctly evaluating them are not yet available. This may, for example, be true of visual figural after-effects. My theory predicts that these should be greater for extraverts than for introverts, *provided both receive an equal amount of stimulation*. The theory would also seem to predict, however, that visual fixation for any lengthy period of time (which is an essential part of ensuring equal stimulation) will be more difficult to maintain for extraverts than for introverts. Thus at present, we appear to have a deduction here which is difficult to submit to any proper test. It may be possible in the future to do so by paralysing the eye muscles, by taking a film record of eye movements, or by the short-time exposure methods at present being experimented with by H. Holland in my laboratory. Until a proper test of the hypothesis is carried out, it is impossible to evaluate the prediction scientifically. Storms and Sigal make much play with Nichols's (1955) failure to observe differences in visual figural after-effects, but as he failed to control for eye movements it is difficult to see how his results can be conclusive one way or another.

When it comes to the verification of deductions actually attempted by various experimenters, Storms and Sigal commit a further methodological error. To put it briefly, they argue as if *failure to disprove the null hypothesis* were equivalent to *proving the null hypothesis*. They regard results which are in the predicted

direction, but which fail to be significant, as actual disproof of the hypothesis in question. This is never admissible, and the only conclusion which can be drawn is that the issue is still open. However, even this conclusion ceases to be very meaningful when the number of cases used is so small as to make the attainment of a significant correlation almost impossible. As an example, a study by Sigal, Star and Franks (1958) may be mentioned, in which they reported a failure to find significant differences between certain neurotic groups on a test of extraversion. The number of psychopaths tested by them, to give but one example, was only eight, and the likelihood of a group of that size being discriminated significantly from other groups, all selected on the basis of highly unreliable psychiatric ratings, is rather small.

A proper test of deduction from a theory must ensure that the number of cases is large enough for a negative result to have some meaning. Storms and Sigal never mention the fact that particularly in correlational research the smallness of the groups employed heavily loads the dice against significant positive findings, and largely invalidates the failure to obtain significantly positive results to be significant as evidence against the theory.

Even more curious is a related complaint by Storms and Sigal, to wit, that there is, on the part of the writer, 'frequent failure to mention relevant test or population characteristics when making a case for the practical utility of his measures or methods. There is notably a failure to mention test-retest reliabilities, which are known to be very low for some tests such as eye-blink conditioning.' It is, of course, true that in my work I have been more concerned with validity than with reliability, particularly as with many of the measures, such as eye-blink conditioning, it is impossible to calculate any very meaningful test-retest reliability. However, granted that the test is valid (and I shall return to this point in a minute), it is inevitable that the test must also be reliable. Storms and Sigal are therefore faced with a dilemma. If the test is valid but also unreliable, then its true validity when

corrected for attenuation must be very high indeed. It is in fact an axiom in psychometrics that the test to go for is that which is valid but has a relatively low reliability, the reason being, of course, that it is reasonably easy to increase reliability, and that any such increase is reflected in increased validity. The only alternative open to Storm and Sigal would be to admit that high validity implies high reliability, and that they were mistaken in the criticism.

We come now to the question of the validity of the conditioning technique. It will be remembered that Franks (1956) used both eye-blink conditioning and PGR conditioning measures on groups of hysterics and dysthymics, finding a total misclassification of only 15 % with the one measure, and 30 % with the other. He also found that 'when a double criterion is used... then the two groups may be separated with no misclassification whatsoever.' What do Storms and Sigal have to say in criticism of this work? Their main criticism appears to be related to initial blink sensitivity, and to a possible higher spontaneous blink rate among dysthymics. These criticisms are difficult to understand. Franks's selection procedure involved giving each subject 'three tone stimuli, followed by three air puff stimuli (not paired with the CS) and then three more tone stimuli. Only S's who did not give PGR or eye-blink responses to the last three tone stimuli were included in the conditioning study. The purposes of these trials were to eliminate those S's who showed any evidence of pseudo-conditioning or original sensitivity to the tone.' It might be thought that Franks was in fact loading the dice against the hypothesis under investigation, because pseudo-conditioning has in fact been shown (Wickens & Wickens, 1942) to be identical in principle with true conditioning, so that by his method Franks would eliminate the most readily conditioned subjects, i.e. dysthymics according to the hypothesis. However, this is not what Storms and Sigal protest against. They say: 'All subjects and therefore all groups had initial scores of zero blinks. However, these zero scores were arbitrarily forced to be

so. It may be that more persons of one group than of the other would blink at least once in another series of three tone presentations. It should also be pointed out that the tone trials previous to conditioning should have consisted of eighteen presentations in order to provide comparability to the test series scores'. These requirements are quite arbitrary and quite unlike anything that has ever been done by other workers in this field. An investigator has to decide at which point safeguards and controls against a particular artifact are sufficient, and he is usually guided in this by what is known about the phenomena and by common practice. If Franks had given eighteen presentations of the tone alone, as Storms and Sigal demand, they might have advanced exactly the same argument as before and demanded 36 or 200 or 5000 presentations. Clearly they are going beyond the limits of rational criticism in this, and making it quite impossible for any study to be acceptable in this field.

Much the same must be said about another suggestion of theirs, namely, that 'at least some of the difference between hysterics and dysthymics in total number of blinks during the test series could be due to a higher spontaneous blink rate amongst dysthymics.' Such evidence as is available from the literature (Meyer, Bahrack and Fitts, 1953) does not suggest any positive relationship between blink scores and data from personality inventories, and Franks himself (1956) found no significant difference between hysterics and dysthymics over a 1-minute period following the conditioning and extinction trials, a test which in fact loads the dice in favour of the Storms-Sigal hypothesis because of the possibility of reminiscence effects.*

* I have not dealt with the Storms and Sigal analysis of variance of rate of growth of conditioning. It is not clear to me what precisely it is they are trying to prove or disprove. The undoubtedly significant differences between extraverts and introverts in total number of conditioned responses, as well as the significant correlation between introversion and conditioning, are sufficient to verify the deduction from my general theory.

It is interesting to contrast the treatment Storms and Sigal give to these conditioning data with their treatment of data relating to visual satiation. They dismiss the very highly significant results of Franks (1956) on the basis of criticisms which are far-fetched and unrelated to current practices of experts in this field. Conversely, they severely criticize the writer for refusing to accept Nichols's (1955) equivocal data on visual satiation because of the very serious doubt relating to the question of fixation. They say that 'no evidence was presented to support this as a valid objection to the acceptance of his (Nichols's) findings'. Again, therefore, Storms and Sigal are attempting to have it both ways. Data favourable to the writer's hypothesis can be dismissed on the basis of highly speculative and unlikely objections, and without any evidence being presented 'to support this as a valid objection'. On the other hand, the writer's refusal to consider data subject to very serious criticism as being crucial to his hypothesis is merely greeted with demands for direct evidence that the suggested objection is in fact true. This certainly is an unusual request to make; in scientific criticism it is usually considered sufficient to raise a reasonable doubt, particularly when this is in line with theoretical consideration, to leave the question *sub judice*; it is not considered essential that the critic himself should provide direct evidence that his objection can in fact be sustained. If this is indeed what Storms and Sigal are suggesting, then clearly most of their criticisms would have to be withdrawn as having no factual basis or support.*

* They fail to mention that the writer's criticism of Nichols's failure to control for fixation differences has also been extended by him to such phenomena as after-effects of rotating spirals (Eysenck, 1957) p. 154, where the reported results are *favourable* and the criticism would invalidate this support of the writer's theories. It is thus far from being made simply *ad hoc*, and in order to explain away a contrary result. Storms and Sigal do not mention this important fact, but prefer to pretend that this criticism was specific to an inconvenient finding.

One of the most important and, if true, damaging criticisms of Storms and Sigal relates to the validity of the Maudsley Personality Inventory. This questionnaire was constructed to measure neuroticism and extraversion, and a considerable amount of work has been done in relation to it (Eysenck, 1956, 1959). Of particular interest at the moment is the relationship of the extraversion scale to different psychiatric groups. The essential analogy here stems from Jung's hypothesis and may be put in the following form: Extraversion/Hysteria and Psychopathy = Introversion/Dysthymia. In other words, on the extraversion-introversion continuum dysthymics will be found towards the introverted side of the other groups, hysterics and psychopaths towards the extraverted side of the dysthymics. This is in fact what Sigal *et al.* (1958) found in a study specially carried out to test this hypothesis, although because of the small number of cases not all their differences were significant. Dysthymics had the highest introversion scores, psychopaths the highest extraversion scores, and hysterics were intermediate with a mean score on the extraversion scale higher than that of the dysthymics. The writer has collected records from a large variety of normal and neurotic subjects, and these have completely borne out the findings of Sigal *et al.*, all the differences being fully significant statistically (Eysenck, 1958*e*). The prediction about the relative position of the dysthymics at the introverted end, and the other two groups towards the extraverted end, is therefore fully confirmed. It is interesting, but not relevant, to add that normal groups, criminal groups and patients suffering from psychosomatic disorders all had scores on the extraversion scale very similar to those obtained by the hysterics. The interpretation of this finding is open to discussion. It is possible that Jung's hypothesis regarding the high degree of extraversion of hysterics may have been mistaken; they may be *relatively* more extraverted than dysthymics, but not *absolutely* so in comparison with the normal population. This would be an interesting finding if true, but it in no way affects the

usefulness of the M.P.I. as a measure of relative extraversion in normal or neurotic groups. In any case, other hypotheses are tenable regarding this finding, but will not be discussed here as they are not really relevant to the point at issue (Eysenck, 1958f). The interesting point in this connexion is the attempt by Storms and Sigal to give the impression that the M.P.I. is not a good measure of the hysteric-dysthymic dichotomy, and has been shown not to be so. This is untrue, and Storms and Sigal have treated this point in a very cavalier fashion in view of the fact that they were fully aware of the points raised in the last few paragraphs.

This failure to mention evidence and arguments contrary to their view (*suppressio veri*), is one which recurs throughout their paper and is, in view of their criticism of the writer on precisely this point, a curious verification of the Freudian mechanism of projection. One or two further examples of this tendency must suffice. One of their first criticisms is the writer's 'failure to cross-validate before considering a measure useful or adequate. An example of the relevance of this is the Eysenck & Prell (1951) study of the inheritance of neuroticism. In this study, the only measures with appreciable loadings on neuroticism were tests of body sway suggestibility, static ataxia and autokinetic movement. Only the first two of these tests have been used in later relevant research, and they have not held up as discriminators of neurotics from normals' (S. B. G. Eysenck, 1955; Hildebrand, 1958). The reader would not guess from this account that a special study was done by Eysenck & Prell (1952) demonstrating highly significant differences between normal children and neurotic children with the use of the factor scores on neuroticism derived from the tests so slightly referred to by Storms and Sigal. Nor would one gather from their account that the work of Ingham (1954, 1955), Connor (1952), Cattell (1957) and many other authors has in fact strongly supported the relationship between neuroticism on the one hand, and static ataxia and body sway suggestibility on the

other. By suppressing the relevant evidence, Storms and Sigal succeed in giving an impression which is quite contrary to fact.*

Much the same is true of their treatment of reminiscence. They criticize the writer for quoting the Treadwell study (1956) as independent duplication, but make no mention of the fact that other duplications, such as that by Star (1957), exist and are familiar to them. In addition to this, they confuse the issue in a number of ways which are of some interest to students of the uses and abuses of communication. Storms and Sigal state that 'Eysenck found a correlation of 0.29 between the E scale of the M.P.I. and the first reminiscence score. This correlation, significant at the two per cent level, he reports in the book; the insignificant correlation of 0.10 between the E scale and the second reminiscence score is not reported.' The impression this may convey to the reader is undoubtedly that there has been some sharp practice here. The reader will not realize from their account that the theory under investigation actually predicted that the second reminiscence score would correlate lower than the first with extraversion, so that this additional finding could in fact have been quoted as further verification of the theory. The failure to do so, as well as the failure to repeat in detail findings reported previously in articles, is due simply to the fact that books have a finite length, and that it would be absurd to repeat all details once these are available in published form elsewhere.

Throughout their review, Storms and Sigal attempt to give the impression that the writer

* Cattell (1957, p. 252) alone quotes *seven* independent experiments of his own in which body sway suggestibility appears as part of his neuroticism factor; there is no other test of his battery which performs as consistently as this. Storms and Sigal may have been ignorant of these facts, in which case they can hardly be considered as well qualified to write a critique of this kind; if they were aware of the facts, but misrepresented them, an even harsher verdict would have to be passed on their qualification for the task they set themselves.

has selectively omitted evidence contrary to his hypotheses, while adducing all the available evidence in favour. The truth of the matter, of course, is that *The Dynamics of Anxiety and Hysteria* was not written as a comprehensive text-book, and that much material both *pro* and *con* had to be omitted for reasons of length. Storms and Sigal always draw attention to omissions of material which might be considered as being *contrary* to the writer's hypotheses, whereas they never draw attention to similar omissions of material *favourable* to the writer's hypotheses such as, for instance, the work of Nichols (1955) on thresholds for apparent movement, which they mention near the end of their paper. This work supports the writer's theory quite strongly, but was not mentioned in *The Dynamics of Anxiety and Hysteria*. No hint of this omission is given by Storms and Sigal, who have noted every other omission which could possibly be construed as being concerned with material not supporting the writer's hypothesis. Such selectivity is fortunately unusual in scientific criticism.

Perhaps a further example of this tendency on the part of Storms and Sigal to make insinuations having little point to them may be in order. In dealing with one of my drug studies in which I tested certain predictions relating stimulant and depressant drugs to pursuit rotor learning curves, they say: 'However, he does not provide data for reminiscence scores, which could easily have been obtained. Inspection of the curves he does provide indicate that the introvertizing drug (dextro-drine) produced more reminiscence than the extravertizing drug (amytal). This part of the data is more directly relevant to the theory, though inconsistent with it, than the parts discussed by Eysenck. Yet he fails to report the result or to indicate whether it is statistically significant.' The statistical test was carried out and failed to indicate any difference between the groups. It was not reported for the simple reason that no prediction can in fact be made from my drug postulate on reminiscence effects where both pre- and post-rest testing is carried out while the subject is still under the influence

of the drug. Storms and Sigal nowhere state precisely how they would derive any such prediction as theirs, and consequently it is difficult to argue the point which appears entirely notional. On the Kimble hypothesis of reminiscence, inhibition rises until it equals drive; thereafter certain involuntary rest pauses are enforced which inhibit work. Drugs may affect the speed of generation of inhibition, or the drive level, or the speed of dissipation, or any combination of these variables; they may also affect sHR. My theory does not permit any choice between these possibilities, which have to be investigated empirically; consequently no prediction can be made about reminiscence effects. The only statement that can be made is the one actually tested, namely, that performance should differentiate significantly between drug effects. This is so because the facts would be identical regardless of the particular variable affected by the drug. This is not true of the reminiscence score, and it is for this reason that no prediction was made. A brief statement of this point will be found on p. 238 of '*Dynamics of Anxiety and Hysteria*'; it is not mentioned by Storms and Sigal. Storms and Sigal do not document their views that reminiscence effects are more directly relevant to my theory than are the effects reported, and that they are inconsistent with it; they rest content with the simple insinuation that the data were suppressed because they were inconsistent with theory. This is a good example of *suggestio falsi*; the alert reader will find many others in the original paper by Storms and Sigal.

One of these examples at least must be mentioned before closing. As no. 7 of their list of 'general deficiencies', Storms and Sigal say that 'when an article is quoted or a figure reproduced items which are not fully consistent with Eysenck's hypotheses may be altered or omitted.' The reader may be interested in looking at one of the instances given by Storms and Sigal. In connexion with the discussion of conditioning, Storms and Sigal say: 'It may be noted that the graph presented by Eysenck contains certain errors which make the groups

appear to differ more than was actually found by Franks.' The figure in question was photographed from Franks' (1956) article for reproduction, but was not found satisfactory by the publisher, who had it redrawn by his own artist. In doing so, the artist made two minute and quite unimportant errors. One was in the direction of increasing the apparent differentiation of the groups during extinction, the other in the opposite direction, during acquisition of the conditioned response. Neither of these two minor errors affects the issue in the slightest, and indeed it takes considerable time in checking the figures to note any differences at all. It is characteristic of Storms and Sigal that they mention the one error but not the other. The reader is invited to verify for himself the completely negligible nature of the error in question.

A similar comment applies to the other example given by Storms and Sigal, to wit, the figure presented by the writer of the two factors emerging from the Trouton and Maxwell (1956) study. It is perfectly true that for reasons of legibility and clarity a number of items have been omitted in figure 3 of *The Dynamics of Anxiety and Hysteria*; Storms and Sigal do not mention that a full list of all the items involved was given on a previous page so that the reader could be in no doubt about the facts of the case, and could consult the original publication if interested in any of the missing items.

One more remark may be added relating to the last sentence of Storms and Sigal's review. They say that 'it is clear that at least Eysenck's typological postulate and probably the one about individual differences require considerable modification to cover the available evidence. This necessity is not suggested by the book....' The reader who will have a look at

what I have to say on pages 250 to 259 will see that I have no doubt myself about the necessity of considerable modification. As I have pointed out there, a good deal of the difficulty of making precise predictions from personality postulates lies in the backward state of so many of the fields to which these predictions could be applied. Even in the most advanced fields like conditioning, reminiscence and so on, there is much that remains doubtful and debatable; as I point out in my Introduction, theorizing in science is an uncertain business at the best of times, and never more so than in the early stages of development of a science. I have stated quite clearly the purpose of presenting my theory. 'In the first place, it may be regarded as a first feeble attempt to achieve the aims of a properly quantified and rigorous system. In the second place, it may serve to bring together, in one set of generalizations, large numbers of more or less certainly established facts. In the third place, such a system may give rise to predictions whose main interest will be not only the verification of the system they make possible, but also the fact that they point to areas of investigation which might otherwise have been overlooked.' It will be clear from this quotation that I am far from regarding my postulates as immutable and certain parts of knowledge. They were put forward as aids to further research and as possible generalizations; I shall be quite content if Storms and Sigal are right in saying that they require considerable modification, rather than complete replacement. I would, however, express a doubt as to whether such one-sided, partial, non-factual and inaccurate critiques as that of Storms and Sigal will be of any great assistance in improving either the accuracy or the coverage of the theory in question.

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MISCONCEPTIONS IN 'SCIENTIFIC METHODOLOGY AND THE DYNAMICS OF ANXIETY AND HYSTERIA'

BY LOWELL H. STORMS AND JOHN J. SIGAL

The title of Prof. Eysenck's rejoinder (Eysenck, 1959) to our comments (Storms & Sigal, 1958) on his book begins with the words 'Scientific methodology' and his principal argument is that we are blind to the essentials of science. He alleges that our critique is 'one-sided, partial, non-factual, and inaccurate', and complains that we regard insignificant results as proving the null hypothesis, that we demand that he test every possible deduction from his theory, and that we have suppressed relevant evidence and presented only findings unfavourable to his theory. Apparently it is necessary to reiterate a very clear statement made in our introduction: 'In order to deal with work deemed relevant to the theory by its author, we have based our discussion upon studies cited in *'The Dynamics of Anxiety and Hysteria'* as favouring or being consistent with the theory set forth therein. This paper will present aspects of the *same* research *unfavourable* to the theory' (italics not in the original article). We further state that disproportionate emphasis may be placed on some topics and, 'we refer the reader to the book, other writings of its author, and references given by him to complete the picture' (Storms & Sigal, 1958). This is hardly consistent with Eysenck's statement that we failed to mention evidence and arguments contrary to our view—a fact which coupled with our criticism of his book 'on precisely this point' he considers to constitute 'a curious verification of the Freudian mechanism of projection'. It is apparent that our attempt to balance the picture by presenting evidence not mentioned by Eysenck when discussing the same research cannot reasonably be construed as meaning that we believe we have proved the null hypothesis. We merely intended to report some aspects of the research which were significant

in the direction opposite to his prediction but not reported by him, as well as some findings, also not presented by Eysenck, which were tests of his theory but failed to confirm or clearly to disconfirm the appropriate predictions.

Throughout his book, Eysenck suggests that the evidence he was able to examine up to the time of writing the book is largely consistent with his theory. We made it a matter of deliberate policy to examine this evidence, limiting ourselves to other aspects of the same data used by Eysenck or other closely related literature stemming from the same period. *Whatever light the scientific literature published since the appearance of Eysenck's book may throw on the data we examined, it will not alter the fact that the discussion of the literature in his book suffers from a marked selective bias.*

Ignoring our stated purpose (to re-examine the research he discusses in the book), Eysenck takes us to task for failing to mention literature published subsequent to the writing of his book (Cattell, 1957; Star, 1957) and not discussed therein, suggesting that we were deliberately suppressing evidence. A careful reader of Star's thesis will discover that his results, too, are far from being in complete agreement with Eysenck's theory. Eysenck also fails to indicate that Cattell's 'neuroticism' is just one of eighteen 'primary source traits', that there is no evidence that it is the same as any of Eysenck's neuroticism factors, and that it has not been shown to discriminate neurotics from normals. It is quite possible that other of Cattell's factors, such as 'anxiety' or 'cortical alertia' are closer to Eysenck's 'neuroticism', depending, of course, upon which of Eysenck's neuroticism factors is chosen.

As Eysenck says, it would indeed be absurd

to demand that all the possible deductions from a theory be tested before the theory is put before the public. The value of an established theory lies in its ability to predict and explain instances other than those which were part of its experimental verification. We have nowhere implied that the theory in *'The Dynamics of Anxiety and Hysteria'* is deficient because all possible deductions from it have not been tested. Our contention is that the tests that *have* been conducted provide less support for the theory than the evidence in the book suggests. We did comment that several very clear implications of the postulates subject to relatively direct test were passed over in favour of more indirect methods. Eysenck chose to interpret such comments as, 'such a straightforward investigation of the relationships which he postulates is nowhere reported by Eysenck', as representing naïve, absurd demands.

In his discussion of specific points in our paper Eysenck has addressed himself to a few, but by no means all, of our more important arguments and to a few points of lesser importance. It is natural that his rebuttal should be directed to those parts of our article regarding which he feels he can marshal the most effective counter-arguments. He has, however, not attempted to answer our questions regarding the definition and measurement of introversion-extraversion and its relationship to hysteria-dysthymia. His reply to our discussion and re-analysis of the conditioning experiments merely repeats points he made in his book except for a discussion of the arbitrarily forced zero points which suggests that he misunderstood our argument, and he claims not to understand the import of the analyses of variance which failed to reveal significantly different rates of conditioning for hysterics and dysthymics. (His Typological Postulate implies that hysterics develop inhibition quickly whereas dysthymics do so slowly. We made and tested what appeared to us to be the simple deduction from this, viz. that the latter should condition at a more rapid rate than the former.) These analyses, which failed

to confirm Eysenck's theory, constituted the major force of our arguments concerning the two learning experiments reported by Franks (1954, 1956a, b, 1957). Even if Eysenck's contention that Nichols's subjects may have failed to fixate is accepted (and we still feel that his 'reasonable doubt' would have been more reasonable had it been supported by some evidence),* the same author's findings regarding apparent movement, horizontal adjustment of lines, auditory localization and discrimination, and kinaesthetic figural after-effect are not called into question by Eysenck yet fail to confirm his predictions. The reader who refers to our paper after reading Eysenck's reply will have no difficulty finding other important questions left unanswered.

In fact, the most helpful suggestion we can make to the reader of Eysenck's rejoinder is to re-read the appropriate portions of our paper. The first of Eysenck's objections brought to light the only point on which we feel we might amend a statement of ours. Under 'Dimensional Basis and General Comments', we noted that no longitudinal studies of his dimensions have been conducted. Perhaps it is unreasonable to expect longitudinal studies to be performed in the eleven years since the appearance of his *'Dimensions of Personality'* (1947); we should have said they 'have not even been proposed or initiated to our knowledge'.

Eysenck states: 'One of the most important and, if true, damaging criticisms of Storms and Sigal relates to the validity of the Maudsley Personality Inventory.' It should be noted that Eysenck claims that we have attempted 'to give the impression that the M.P.I. is not a good

* One wonders why Eysenck should consider a 'reasonable doubt' applicable only to his criticism of the work of others but not to criticisms made of his work. For example, we feel we have cast 'reasonable doubt' on Eysenck's explanation of Nichols's visual satiation data by pointing out that Nichols provided no evidence of difference in eye fixation and by citing Eysenck's own statement that Granger (1957) found no such differences in a dark adaptation experiment.

measure of the *hysteric-dysthymic dichotomy*' (italics ours). The E scale of that test purports to measure extraversion-introversion, not merely hysteria-dysthymia, and it was as a measure of I-E that we questioned its validity. A thorough and adequate rebuttal of his argument has been presented elsewhere (Sigal, Star & Franks, 1958*b*), but several points deserve emphasis here. In view of the fact that his larger sample yielded the same results as that of Sigal, Star & Franks (1958*a*), Eysenck's argument regarding sample size appears pointless, especially since the smallest of the Sigal *et al.* subsamples (psychopaths) produced the result most consistent with his theory, a result one would not expect him to criticize. His 'Typological Postulate' states that persons who develop inhibition rapidly 'are thereby predisposed to develop extraverted patterns of behaviour and to develop hysterical-psychopathic disorders in case of neurotic breakdown...', and table 6 of his book states that hysterics are extraverted and dysthymics introverted. This is obviously a statement of absolute, not merely relative, position. It clearly implies that hysterics should be more extraverted than normals, whom Eysenck has called ambiverts (1953, p. 30). Now Eysenck says his new data 'have completely borne out the findings of Sigal *et al.*, all the differences being fully significant statistically', and he adds that his normal groups 'all had scores on the extraversion scale very similar to those obtained by the hysterics'. If on the basis of these data which place hysterics in the middle of the I-E continuum as measured by the E scale, Eysenck is justified in saying that they are 'toward the extraverted end,' then it is equally justifiable to say that, relative to psychopaths they are 'toward the *introverted* end' and, hence, are introverts, relatively speaking. Both these statements are clearly in direct contradiction to his theory as stated in '*The Dynamics of Anxiety and Hysteria*', as well as being contrary to his expectation that if diagnostically pure hysterics and dysthymics were used in experimental studies the whole I-E range would be covered (e.g. Eysenck,

1957, p. 31). Lacking direct validation studies of the E scale as a measure of I-E, we can merely repeat our original statement, viz. 'one is forced to conclude either that the Typological Postulate is wrong or that a measure of I-E appropriate to the theory has not been found, or both.' Eysenck is aware that these are the only alternatives open to him, but has not yet decided which to accept for he only concludes from the data that hysterics *may* not be (absolutely) extraverted. However, any of these alternatives is, to use his words, 'important and...damaging...'.

Acknowledging the distortions we discovered in two of his figures, Eysenck disclaims responsibility for one of them and dismisses them both as inconsequential. It is an accepted principle in scientific circles that *an author is responsible for the accuracy of his text, figures and captions*. If a figure is altered in any way, it must be so stated. In discussing his version of the factors represented by the Trouton & Maxwell (1956) figure, Eysenck emphasizes the importance of 'the demonstration that they are orthogonal.' Since orthogonality is a mathematical artifact unless oblique solutions are also tried and the orthogonal ones found to fit best, the neat clustering of the *selected* items around the axes seems his only basis for claiming a demonstration of orthogonality. The omission of items from the figure is therefore hardly inconsequential. We urge the reader to accept Eysenck's invitation to the reader to 'verify for himself' the nature of the other error. Despite Eysenck's contention to the contrary, the total effect is to make a somewhat uncertain trend appear more consistent in favour of his predictions.

SUMMARY

Eysenck's reply to our article consists chiefly of three kinds of evasion:

1. *Groundless complaints that we have played fast and loose with scientific methodology* e.g. that we failed to report data favourable to his theory and implied we had proved the null hypothesis. In fact, we had clearly stated

our intention to bring to light unfavourable aspects of the same research of which Eysenck had already discussed the favourable portions. We had also said in our introduction and in our section on conclusions that Eysenck's work should be consulted to round out the picture. We were not so misguided as to try to 'disprove' his theory by 'proving the null hypothesis,' but we did succeed in showing that it is somewhat less certain than Eysenck indicated that the theory covers the empirical findings which are considered to be explainable in its framework.

2. *Answering questions that were not asked or focusing on unimportant details in order to avoid discussion of the issues raised in our paper.* We have already described this type of handling of our comments regarding Franks's conditioning studies, Nichols's work on after-effects, and the problem of the validity of the M.P.I. His tactics for handling our questions

regarding the drug studies (e.g. Would not the apparent effect of the drugs on neuroticism make it difficult to determine whether differences between drug groups are due to neuroticism or extraversion?) consisted largely of diverting the reader's attention to a long and inconclusive discussion of our most minor point. His argument about the relation between validity and reliability of tests begged the question by assuming that validity has already been demonstrated. Our entire point was that this is not so until cross-validation has been done—an objection not answered by Eysenck.

3. *Speculation in the form of adjectives supposedly descriptive of our personalities and motives.* This may have served to distract attention from the central issues of our paper but seems of doubtful relevance or merit in a discussion allegedly dealing with problems of scientific methodology.

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CRITICAL NOTICE

By EDWARD GLOVER

Psychotherapy by Reciprocal Inhibition. By JOSEPH WOLPE, M.D. (Pp. xiv+239. 40s.) California: Stanford University Press. 1958.

Dr Wolpe's book is almost as nostalgic as the scent of a late Victorian wardrobe. It is, to say the least, not far short of half a century since debates regarding the nature of psychic phenomena turned on the heuristic virtues of neuro-physiology and of clinical psychology respectively. Significantly enough these exchanges were not usually sustained, either by champions of the conditioned reflex or by psychoanalytical metapsychologists. For your true-blue Pavlovian, naturally convinced that he had a physiological right of way to mind, was not really interested in the convolutions of pure psychology, and your dyed-in-the-wool psychoanalyst, cleaving determinedly to his working concepts, felt no incentive to concern himself with what he held to be the relatively barren wastes of physiological thinking regarding mind. On the other hand, two groups in particular found themselves on the horns of a dilemma: those who, however elaborate their techniques or broad their clinical experience, had no systematic theory of mind to guide their thinking and were therefore liable to slip over the boundaries of psychology into whatever non-psychological approach seemed to offer a rational explanation of their patient's troubles; and those who for one reason or another were or became allergic to psychoanalytical theories or practices, and turned with zest to the more naïve concepts of physiology.

Dr Wolpe clearly belongs to the latter group. By his own confession 'a staunch follower of Freud' as recently as 1944 (although to judge from the Freudian quotations in this book his staunchness seems to have been in inverse ratio to his grasp of psychoanalytical theories) he apparently recognized the error of his psychoanalytical ways and executed a strategic retreat, via Pavlov and Hull to neurophysiology, flirting on the way with the Jungian concepts of introversion and extraversion; as a result of all of which he finally arrived at 'the new methods of psychotherapy' which he is concerned to set out in the second part of his book.

In passing it may be remarked that these ideological tides which are responsible for a sort of Nietzschean recurrence in the theories of clinical psychology, or, in more homely metaphor, the periodic affixing of new labels to old bottles, are by no means unfamiliar to the psychiatric historian. Perhaps this is due to a peculiarly psychiatric need to advance every now and then 'new' systems for 'old', even if the 'new' system is usually mutton dressed up as lamb. Perhaps it is reinforced by the psychotherapeutic nihilism which still pervades psychiatry and gives rise to a therapeutic hunger for allegedly physiological 'short-cuts'. Moreover, we have recently observed somewhat pretentious systems of 'interpersonal relations' sweep the psychotherapeutic field without disclosing too freely their relatively ancient 'transference' lineage. Even psychoanalysts now and then, and perhaps more commonly in the U.S.A., rediscover Freud, and deck their rediscoveries with unfamiliar terminology. For the matter of that there have not been wanting psychoanalysts prepared to eke out or support their psychoanalytical reconstructions with apparent parallels drawn from embryology, neurology, endocrinology and 'animal psychology'. Physiologists it would appear, have so far been more immune to this hunger for the new. On the whole 'learning theory' has not strayed very far from its original molecular behaviouristic basis and laboratory locus. But that may be only a matter of time. Judging by these present portents, to say nothing of the activities of the Eysenck* group in this country, we may well be about to witness laboratory psychology seeking to spread its fledgling wings over the psychiatric field.

However that may be, Dr Wolpe's dilemma is clearly indicated by the title he has chosen for his book. For either he means by it that psychotherapeutic aims can be achieved by the application of neurophysiological principles or he means that

* H. J. Eysenck, *The Dynamics of Anxiety and Hysteria*. London: Routledge and Kegan Paul. 1957.

the application of neurophysiological principles itself constitutes psychotherapy. Even his choice of clinical material does not help to solve this dilemma. For while he applies his methods to conditions which were finally isolated on purely psychological criteria (*'psycho-neuroses'*, for example) he is equally clearly concerned with conditions which the physiologist originally termed *'neuroses'* as distinct from *'psychoses'* or mental disorders.* Certainly in view of his concern with *'unlearning'* physiological responses it would have been less confusing to the reader and more consistent with his own theories if he had made no bones of the matter and had entitled his book *'The Treatment of Unadapted Behavioral Responses by Reciprocal Inhibition.'* Even so it would not have solved his theoretical dilemma, which is essentially the dilemma of the epiphenomenologist who finds himself unable either to discard or to discount his epiphenomena.

Under these circumstances it is justifiable to consider his presentation in inverted order, to examine first his therapeutic techniques and subsequently the particular form of *'metaphysiology'* with which he attempts to elucidate them. This can best be done by examining closely his *'Approach to the Patient'* (pp. 105-13), for all that follows is merely a repetition of the main physiological thesis set forth in this short section. The first step is apparently to obtain a symptomatic history with one eye on possible precipitating factors. Should difficulties arise in this quarter Dr Wolpe ceases to press his points and at the same time fires the first salvo of his suggestion procedures, informing his patient that it is not necessary to rake up past history.

Having thus thrown his weight on the side of reactive anti-cathexis, the author's subsequent behaviour may seem to the psychologist somewhat inconsistent in that he spends the next ten or so sessions taking *'The patient's life history'* in a manner reminiscent of the conventional approaches of general psychiatry, stopping every now and then to counter by suggestion any conflict effects (e.g. conscious guilt) he may encounter.

* Incidentally later research showed that, purely organic nervous disorders apart, many of these *'neuroses'* were actually *'psychosomatoses'* derived from *'functional disorders'* of the mental apparatus.

Of course Dr Wolpe is unaware of any inconsistency; indeed his avowed aim is to determine what stimuli do or can evoke neurotic reactions in the present day. These stimuli it is his intention to counter by encouraging antagonistic responses.

Having then applied the Willoughby questionnaire (a test for *'neuroticism'*, that most confused and confusing of all clinical captions) Dr Wolpe fires his second and more extensive and intensive broadside. Following the techniques of pure suggestion he delivers a lecture to his patient on the nature of anxiety, real or unreal, and informs him that the essence of his treatment is to oppose his anxieties with other emotional states incompatible with them. With this preliminary *'dressage'* the ground is prepared for more specific measures of suggestion.

Space does not permit a detailed survey of the various types of response cultivated by Dr Wolpe or the various clinical conditions or syndromes to which he applies them; but it is of interest to list some of the main and some of the auxiliary techniques he employs. In addition to persistent measures of suggestion and exhortation, Dr Wolpe has recourse to deep hypnotic suggestion, relaxation techniques varying from the laying on of hands to what might be called thematic hypnosis, lectures on sexual hygiene, carbon dioxide therapy, the application of electrical shocks of varying severity up to the limits of physical tolerance, *'psychotherapeutic interviews'*, or anamnestic therapy as it might be called, *'abreaction therapy'*, together with such auxiliary devices as *'correcting misconceptions'*, *'thought stopping'* under command, and the exhibition of drugs such as pentothal, codeine and the like. These methods he applies either singly or in various combinations and sequences.

So there we are, back over forty years to discussions of the nature of suggestion in which the author with one eye somewhat balefully regarding the existence of psychoanalytic therapy keeps the other steadily fixed on the physiological principles of reciprocal inhibition. For good measure and in case the psychoanalyst should every now and then claim therapeutic successes, Dr Wolpe maintains that these, indeed successes in any form of psychiatric therapy, are due to unremitting application on the part of the psychoanalyst or psychiatrist of measures of reciprocal inhibition. A Roland for an Oliver, surely, since the more

partisan amongst analysts have been nothing loath to maintain, that outside the field of psychoanalytic technique, all psychotherapeutic measures have partaken of the nature of transference suggestion, an ex-cathedra pronouncement which Dr Wolpe's therapeutic behaviour goes some distance to corroborate. Indeed if time permitted it would be instructive to subject Dr Wolpe's methods to a detailed analytic interpretation in terms of what are now commonly called 'interpersonal relations' with special reference to the boldness with which, seeking a physiological safe conduct, Dr Wolpe plays about with transference phenomena. More to the point is some consideration of the question how far his methods constitute a sharper focusing of suggestion methods along the lines indicated by the 'hypnoanalysts' and 'narcoanalysts' who, under the illusion that they were amplifying, sharpening and accelerating the technique of psychoanalysis, were in fact experimenting with focally directed or thematic hypnosis and narcotization.

Here unfortunately we encounter an obstacle common to such evaluations, viz. that Dr Wolpe's empirical support of his clinical contentions depends mainly on his estimations of therapeutic results. This is an old and open sore in the psychotherapeutic body. It has to be admitted that the diagnostic, therapeutic and statistical (including after-history) standards employed by every variety of psychotherapist are wretchedly inadequate. As a matter of fact Dr Wolpe's own diagnostic and after-history evaluations are not too disciplined and his comparisons with recorded psychoanalytical results are quite uncontrolled and indeed uncontrollable. Of his last series of 88 cases not less than 21 are recorded simply as 'interpersonal anxiety', whereas the cases examined in the Report of the Berlin Psycho-Analytical Polyclinic (to which he exultantly refers) included some of the most intractable in the clinical calendar, a state of case-selection which applies also to the published records of the London Clinic of Psycho-Analysis, indeed to most records of cases treated privately.

But it is fruitless to bandy results. A mental theory may originate in observations made during psychotherapy, but no mental theory was ever either proved or disproved by therapeutic results obtained through experiments in suggestion. All we need note is that of 210 cases, of whom 135 appear under the caption 'anxiety state', 39 %

were 'apparently cured' and 50 % 'much improved'. Taken at face value this is the kind of report that causes searching of heart amongst those who pin their faith to exploratory and interpretative methods of therapy, and is calculated to make the more faint-hearted of them take to transference suggestion. As a matter of interest similar claims for the efficacy of their treatment come nowadays almost exclusively from the suggestion schools. Psychoanalysts, having spread their therapeutic net from the more straightforward psychoneuroses to include most varieties of character or sexual disorder however deep-seated, and a variety of larval psychoses, have gradually and through bitter experience of failure learned the truth of Freud's cautionary comments regarding the limitations of psychoanalytic therapy and have come to recognize the importance of soundly based prognostic criteria (of suitable selection in fact) and consequently to reserve their exultations for the marginal successes they can sometimes achieve with the more intractable or inaccessible cases. Psychiatric practitioners are also reasonably modest in their psychotherapeutic claims and in any case tend to play for safety by the simultaneous application of neurophysiological modes of treatment ranging from pharmacological or electrical devices to neurosurgery.

Scanning Dr Wolpe's work for techniques that might regulate systematically the application of his suggestion procedures and possibly account to some extent for the successes he claims in anxiety cases (either functional or psychoneurotic) one can only single out the psychological system he applies in 'Systematic Desensitisation based on Relaxation'. This is based on the construction of *thematic* (epiphenomenological) hierarchies to which the patient reacts with graded amounts of anxiety. These psychological themes, whether realistic or merely phantasy displacements, are obtained by history-taking, by psychological questionnaires, and are amplified and graded by the patient in co-operation with the psychotherapist. Proceeding then from the weakest to the strongest 'anxiety stimuli' and in most cases with the help of hypnotic trances the patient is gradually 'desensitised'.

Unfortunately for the author's theories the scientific trail here is blurred by the very multiplicity of his dressage techniques and the modes of suggestion employed. All we can conclude is that

although the thematic method has long been practised by exponents of hypnotic suggestion it differs from the techniques of so-called 'hypnoanalysts' in so far as the latter exploit their hypnotic trances to establish *unconscious* elements in a thematic hierarchy which they subsequently seek to analyse. In so doing the hypnoanalyst gives assent to the theory of repression. Repression, however, is anathema to Dr Wolpe, who, as has been suggested, unwittingly seeks to reinforce it by his 'here and now' limitation of his investigations to conscious content, investigations which, interestingly enough, are faintly reminiscent of the transference interpretation techniques of some latter-day analysts, although, to be sure, Dr Wolpe is not concerned with the transference aspects and unconscious origin of the analytical 'here and now'. As far as this present work is concerned the word 'transference' is on his *index expurgatorius*.

To sum up the clinical aspects it can fairly be said that regarded as an experiment in the application of neurophysiological principles to psychological problems his presentation is marked by a complete absence of focal control, or, more specifically, that the molecular and atomistic procedures he applies mostly to anxiety cases, sometimes of a superficial type, are buried in a mass of molecular and molar suggestion. Dr Wolpe, whether he recognizes it or not, has not escaped from his psychosomatic dilemma. Or rather he has escaped it through an act of faith. All that is left is an affirmation or neurological hypothesis that 'reciprocal inhibition has a central role in psychotherapy'.

Under these circumstances it is not surprising that the first part of Dr Wolpe's book, which is significantly labelled 'Background', should consist largely of a kind of 'dressage' similar to that he employs when softening up his patients prior to the application of more specific measures of suggestion. Covering a variety of approaches, e.g. some anatomical relations of behaviour, the making and unmaking of functional neural connections (learning and unlearning) and the experimental induction of animal 'neuroses', this background leads Dr Wolpe to the same conclusion as do his clinical experiments, namely, the therapeutic success of techniques 'based on reciprocal inhibition principles'. To this is added a final chapter on the etiology of human neuroses in which these disorders are described as, holding the views he

does, he must perforce describe them, as the result of physiological learning processes.

For the clinical psychologist this is the crux of the matter. In his Presidential Address to the British Psychological Society, Alec Rodger* maintained that psychology is profitable regarded as a 'residual' science dealing with physiological 'left-overs'. This pessimistic and somewhat lopsided if not positively unimaginative pronouncement (which Rodger is cautious enough to qualify by saying that physiology may *never* succeed in taking us as far as we want to go in the explanation of human behaviour) does at any rate leave room even if only temporarily for a science of psychology. For Dr Wolpe psychology is not even a 'residual science'; it is subsumed under physiological 'learning', in other words the modification of synaptic transmission of stimuli giving rise to characteristic behaviour responses, adapted or unadapted. The issue for the clinical psychologist is therefore clear enough. Does this assumption adequately account for the data ('normal' as well as 'disordered') which are either directly observed by himself and others or communicated to him through his subject's introspections? If not, do the basic mental concepts with which he operates provide more adequate hypotheses?

Clear understanding of this issue will greatly alleviate the confusions to which the unwary reader is liable to become a prey. He must distinguish clearly between, on the one hand, experimental observations made mostly under laboratory conditions on dogs, cats, mice, rats, sheep, goats, pigs, chimpanzees, or for that matter on humans, and on the other, the definitions, postulates, hypothetical constructs and theories of adapted or unadapted function which when correlated and systematized by Dr Wolpe lead to a purely physiological hypothesis of human behaviour. We may indeed take for read such molecular observations on animals as are generally validated by competent physiologists, without necessarily assuming that they would account for such an apparently simple phenomenon as, for example, a slip of the tongue, as satisfactorily as, according to the psychoanalyst, does the Freudian theory of normal unconscious mental function. To those who restrict themselves to conscious evaluations the slip of the tongue is clearly an unadapted response, to

* *Bulletin of the British Psychological Society*, no. 36, 1958.

those who postulate unconscious forces and mechanisms it is an exquisitely adapted response, one moreover that can perform ventilating, signalling and in the molecular sense of the term, auto-therapeutic functions.

To be sure it is difficult for the psychologist when reading Dr Wolpe's review of experimental 'neuroses' in animals not to indulge in marginal reflexions. He may wonder, for example, why the physiologist faced with characteristic *differences* between man and the lower mammalia, or for that matter the invertebrates, should pin his faith to a simple sort of theriomorphism or, to use non-technical language, should be so concerned with identities rather than with differences in function, or why he should be satisfied to explain away fundamental differences by attributing them to mere complexities of elementary physiological processes. He may speculate as to the number of days, weeks, or even months, of post-natal life during which some correspondence might be held to exist between human and animal behaviour; or, to use his own terminology, the period at which psychosomatic responses to stress constitute the main accumulation and discharge system of the infant human. And he will certainly hold that the equation of 'neuroses' in animals with what, again using his own terminology, he calls the 'psycho-neuroses' of humans is a retrograde and unimaginative step. That, after all, is Dr Wolpe's affair, although he must not be surprised if clinical psychologists feel that his passion for synthesis has played havoc with his clinical perspicacity.

But it is mainly when we come to examine Dr Wolpe's definitions or postulates that we begin to realize the magnitude of the sacrifice of painfully acquired psychological insights we are asked to make if we accept his propositions. This can be readily gathered by studying his preliminary definitions of 'stimulus', of 'need' and 'drive', of 'habit' and of 'environment'. A 'stimulus', for example, is defined as the '*antecedent* of a neuro-effector response' [reviewer's italics], which of course excludes the examination of stimuli that are apparently not followed by any neuro-affecter response. 'Needs', defined (or perhaps it would be better to say recognized) in accordance with conscious feelings of 'wanting' or 'unsatisfaction', are also judged by their neuro-effector response. Therefore 'needs' cannot be absolutely distinguished from sensory stimuli. And so Dr Wolpe

evidently thinks. 'Every sensory stimulus has the essential characteristic of a need.' 'Drive' is defined as a state of excitation of neurones in the central nervous system. A 'habit' is 'a recurring manner of response to a given stimulus situation'. 'Environment is a collective term referring to everything to which the organism is responding at a given time.' Objects 'in a physical proximity' to a subject who is apparently not responding to them at a given time do not constitute 'environment'. They are, Dr Wolpe says, better characterized as 'surroundings'.

No doubt normal or academic psychologists could, if so disposed, have much to say concerning the relation of such definitions to, for example, the conscious problem of 'inattention' or 'negative attention' or 'disattention'; but regarding these and other academic matters we may well leave Dr Wolpe to the tender mercies of the expert in normal conscious psychology, who, even if he should regard his field of activity as a 'residue' so far unexplained by physiology, does at least recognize the heuristic value of mental concepts, however simple. What the clinical psychologist should recognize is that in these first few fell swoops Dr Wolpe would abolish the Freudian distinction between 'instincts' (in the sense of *triebe*) and sensory stimuli, and thereby make nonsense of the invaluable Freudian description of the development of reality-sense, to say nothing of the development of discrimination between the ego and its objects. He would reduce once more to incomprehensibility the phenomena on the study of which the concepts of the 'dynamic unconscious' and of 'repression' are based. He would flatten the Freudian distinction between positive indications of instinctual tension and the reactive tensions following instinctual frustration, also between the pleasure and the reality principle. He ignores the characteristic mental mechanism of projection whereby objects 'in physical proximity' may acquire an excitatory value for the subject which is not intrinsic in their nature, so that, for example, a harmless piece of wire protruding from the sitting-room wall may set the paranoiac into a state of demented excitation. But then Dr Wolpe is not concerned here with a theory of neural function that explains the psychoses; and excludes the unadapted habits of schizophrenia on the grounds that its primary basis is an abnormal organic state. He also ignores the fateful blurring of ego and object which is formed by the mechan-

isms of introjection and of identification. And, to choose but one clinical instance, by his definition of 'habit' he would eliminate the subjective factors of unconscious excitation and conflict which, in the case of the obsessional neurotic, govern his apparently unadapted reaction to an apparently environmental stimulus. Dr Wolpe would in short have us adopt a nursemaid's view of habit without however adopting the nursemaid's prejudice (which is of course a one-sided and rather moralistic insight) that the unadapted behaviour of small children is a measure of their original sin. The word 'guilt' (either conscious or unconscious) is evidently not significant enough to appear in Dr Wolpe's index.

'And a good job too', Dr Wolpe no doubt said to himself when surveying his preliminary efforts at demolition of metapsychological concepts. It would indeed be a work of supererogation to follow the various simplicities of thought which constitute Dr Wolpe's working hypotheses; e.g. his view that the character of behaviour depends on the paths that neural excitation takes, a view which apparently he thinks *explains* behaviour. Which is tantamount to saying that the clenching (or muscular contraction) of fists explains the state of rage directed at a specific object. It is perhaps sufficient to say that the same naive characterizes Dr Wolpe's definitions of neurotic behaviour. Having begged the question by maintaining that neurotic behaviour is a persistent unadapted habit acquired by learning (which *a priori* means the acquisition of new synaptic distributions of neural energy) he singles out as one of the consequences and thereby one of the stigmata of unadapted behaviour viz, the amount of expenditure (presumably he implies here waste) of energy, a criterion which would put a passionate hobby in the same pigeon-hole with a 'neurosis'.

And again, although Dr Wolpe unwittingly flirts with the concept of the pleasure principle when he maintains that 'adaptive consequences take the form of progress towards the satisfaction of a need' or that 'behaviour is adaptive to the extent that it is "worth while"' he seems either to have forgotten the existence of masochism or to be ready to include suicide among the forms of adapted, i.e. non-neurotic behaviour. Yet almost in the same breath he informs us that adapted behaviour leads to the 'avoidance of possible drainage or deprivation', a view which if generally accepted would involve the repeal of the Mutiny

Act or at least subvert the morale of our fighting forces. In passing, no analyst has ever maintained, as Dr Wolpe suggests in a footnote, that 'patients want their neuroses'. This popular misunderstanding, he naturally remarks, is absurd: but if during his period of 'staunch adherence', he had read Freud more carefully he would have realized that this comment is not original. Secondary gain is not a habit acquired through conscious volition, nor is it a primary motivation of neuroses.

Regarding Dr Wolpe's etiological systems and clinical differentiations little remains to be said: the former are based on his learning theories which he takes occasion to recapitulate; whilst his clinical psychology (somewhat remarkably in view of his practical experience) has many resemblances to that advanced by Eysenck. It is over-simple and at the same time confused and lacking in particular any discrimination as between precise clinical entities. His generalized definition of neurotic behaviour which is made to include any persistent habit of unadaptive response not caused by conditions the primal basis of which is organic disease (including in this last category schizophrenia), is a masterpiece of evasion of the duties of a clinician. By this token, the term 'neurotic behaviour' would become a synonym for 'abnormal psychology'. His equally vague category of 'interpersonal anxiety' runs this a good second. Moreover, in his easy acceptance of such wide Jungian concepts as extraversion and introversion, he also follows Eysenck, and with a similar motive, namely, to distinguish between their respective patterns of neural activity, not, as one might reasonably expect, to study the 'interpersonal' nature of 'interpersonal anxiety'. However we may hear more of all this in a projected volume which is apparently to cover a wider clinical range.

Two final reflexions may however be permissible. First, that some of the practical applications of 'learning theory', though perhaps more detailed in scope and not so glaringly moralistic in intent, do not differ in principle from the earlier Victorian and sometimes Edwardian experiments in therapeutic conditioning. This is especially true of counter-conditioning through shock and suggestion. Perhaps the best example is that of Gwynne-Jones's treatment (based on Bykov) of frequency of micturition by the use of a cystometric apparatus in order to apply varying pressures of saline injected into the bladder, using at the same time a manometer the readings of which can be

falsified. Though plainly a technique of counter-suggestion, this does not differ in principle from the Victorian habit of trying to prevent masturbation, when all moralistic and intimidating suggestions had failed to 'extinguish' the 'habit response', by inserting a safety-pin in the prepuce. And it has the same penal significance which lies behind the application of the death penalty for murder, the therapeutic difference being that the latter conditioning process extinguishes the patient as well as his unadapted synaptic responses, and thereby finally cures him of the homicidal habit.

The second reflexion concerns the occasional psychological insights which Dr Wolpe himself achieves but which he nevertheless studiously neglects in formulating his favourite theories. Perhaps the most significant of these concerns the evocation of 'interview-induced emotional responses' antagonistic to anxiety. 'These', says Dr Wolpe, 'arise in some patients in response to the very presence of a situation in which they entrust their happiness to the special knowledge and skill of another human being.' Fortified by this insight into the nature of interpersonal relations, if not of transferences, Dr Wolpe might well have written a good book on suggestion without benefit of neuro-physiology.

It remains to justify the seemingly excessive length of the present survey. If Dr Wolpe's book is so riddled with naïve thinking why, the reader may ask, give him more than a few lines of cursory

notice. In the first place although laboratory-minded normal psychologists are, with outstanding exceptions such as the late J. C. Flugel, inevitably poor clinicians (largely because they neglect the factor of mental suffering) and notoriously inept and unapt in psychotherapy, Dr Wolpe bases his argument regarding the virtues of 'Reciprocal Inhibition' on *clinical* observations and is likely therefore to appeal to those eclectic psychiatrists who are eclectic because they have no theory of mind to guide them beyond empiricism. And secondly, as was suggested at the outset, those psychiatrists who are half-hearted in their techniques and unhappy about their therapeutic failures are prone to embrace with enthusiasm any method that appears to be at the same time less exacting and (if Dr Wolpe, despite his uncontrolled comparisons, is to be believed) more promising than psychotherapy. Already there are indications that the Maudsley, Pavlov *cum* potted Jung School will acquire not a little prestige amongst the more suggestible or timid of psychiatrists, and that this invasion of clinical psychiatry by normal psychologists bearing laboratory instruments in their hands and a deep psychological impermeability in their heads will produce more than a dent in the perimeter of classical psychiatric defences. From this point of view Dr Wolpe's book is no mean essay in supportive suggestion for faint-hearted psychiatrists and deserves to be accurately placed in a hierarchy of contra-psychological approaches.

CLINICAL RESEARCH IN SCHIZOPHRENIA—THE
PSYCHOTHERAPEUTIC APPROACH

BY JAMES CHAPMAN, THOMAS FREEMAN AND ANDREW MCGHIE*

Periodically patients are encountered who can give a detailed description of their complaints and at the same time report upon their subjective experiences as they bear upon the symptomatology. The patient whose history forms the nucleus of this communication belongs to this category. His ability to communicate during psychotherapy has provided the authors with a further opportunity to extend their knowledge of the phenomena which may be found in certain cases of schizophrenia. This information lends support to previously published views (Freeman, Cameron & McGhie, 1958; Freeman & McGhie, 1957), regarding the abnormal mental processes which occur in this disease. The significance of this material for the theory of schizophrenia can be more fully appreciated if one-sided interpretations of the phenomena are abandoned. A lead in this direction was given by Schilder (1924), whose writings reflect his efforts to find a common ground for the appraisal of the psychological and organic factors in mental illness. On the psychological and side current developments in experimental and genetic psychology and the recent emphasis upon ego psychology in psychoanalysis have brought Schilder's endeavours a step nearer to realization.

Rapaport (1951) has pointed out that it was Schilder (1924), once again, who drew attention to the need for study of both the formal or functional aspects of mental processes as well as the contents by which they represent themselves. Schilder's (1924) theories concerning autonomy of cognitive processes has been forcefully revived and strengthened by Hartmann's (1939) hypothesis of autonomous ego functions. The realization that perception,

thinking, motility and memory arise from sources independent of the instinctual drives has had important implications for the psychopathology of schizophrenia. It follows that these functions are not wholly at the mercy of conflicts which can be traced back to a clash between bodily needs and the environment. The unconscious phantasies which come to represent these conflicts can no longer be regarded as the sole cause of the ego defect which is to be found in the psychosis. The need for further knowledge of the developmental phases through which the ego functions pass has led to an awareness of the importance of Piaget's (1947) work on the intellectual growth of young children and to the study of recent work in experimental psychology (Werner & Wapner, 1952).

The defect which exists within the mental organization in schizophrenia does not necessarily extend throughout its entirety. Freud (1914), for example, listed among the phenomena to be observed in schizophrenia those features representing what remains of the normal personality as well as those derived from the phases of withdrawal and restitution. It is the authors' opinion that in the psychotherapy of schizophrenia there has been insufficient differentiation between those phenomena which arise from the psychotic part of the patient's mind and those which spring from the non-psychotic layer. Conflicts which exist in the non-psychotic layer are indeed no different in form or content from the unconscious conflicts which can be observed in the psychoanalysis of the psychoneurotic patient. Localizing such mental processes within the psychotic part of the mind had led to the belief that mental conflict with ensuing defensive activities are as fundamental in the psychotic reactions as in the psychoneurosis. In this presentation the

* Manuscript received 28 November 1958.

authors hope to show that such defensive conflicts, as expressed for example in the transference situation, originate in the non-psychotic part of the patient's mind and thus cannot be regarded as the primary source of the schizophrenic psychosis.

CLINICAL MATERIAL

History and development of symptoms

The patient, a young apprentice in Chartered Accountancy, was admitted to hospital in January 1958, at the age of 23 years. He was the firstborn of two children. There was nothing special to note in his early development. He proved to be a bright, cheerful child at school, taking second place to the Dux pupil in his primary school. From the age of 11 years onwards at High School, his academic performance was maintained at an equally high standard and at the age of 17 years he obtained the School Leaving Certificate without any difficulty. On leaving school at 17 he embarked on a career of his own choosing, that of chartered accountancy with a City firm. For the first five years his performance was beyond reproach and he was successful in all the preliminary examinations. At that time he was cheerful and content, and his relationship with his colleagues was very friendly.

After five years of arduous training his illness became manifest at the beginning of 1956, at the age of 22 years. The initial change was a general slowing up and impairment in efficiency in carrying out all his usual activities, both at work in the office and at home. There was gradual but progressive withdrawal of interest from routine duties and from his studies at home, and at university classes. At this time he complained to his mother that he had 'reached saturation point'. He had previously been very punctual and meticulous in his performance, but now gradually lapsed into habitual lateness for work, and carelessness in carrying out routine tasks. No longer did he apply himself conscientiously to his studies for the final examination. At home he spent many hours staring at the contents of a room,

'checking' them in his head. In contrast to his usual behaviour of having a separate place for everything, he frequently muddled up his own personal possessions in various places, including his pockets, and he resented his mother's attempts to tidy things up for him. He did not like anything to be changed.

These symptoms continued to progress for a year, at the end of which time, early in 1957, disorders of motility became the prominent feature of the illness. When setting out for work, by this time being constantly late, he began to stop and stand still at street corners, aimlessly looking about for 5-10 min. A few weeks later, he stopped going to work altogether, and thereafter, for a period of one year, he remained at home and did not leave the house except on one occasion for a few hours only. Apart from reading motor-car magazines for the first few months, he did not engage in any purposeful activity of any kind, either inside or outside his home.

He preferred to stay up very late at nights, making no effort to go to bed. When finally, after long delay, he got to bed, he slept well but stayed in bed until near noon each day, lying motionless, though awake. He appeared to have difficulty in sitting down in a chair and would stand looking at it for 15-20 minutes before actually managing to occupy the chair. Whenever he was seated, he remained in the same position for several hours without attempting to get up. In general he preferred to remain upright and would each day stand rigidly in the same spot for periods varying from 1 to 3 hours, making no response to any form of communication from his parents. He took an excessive amount of time to carry out ordinary and even trivial tasks, involving movement. When washing his hands he let the water run for about an hour with an attitude of complete indifference. In the mornings, lying in bed, he appeared to be listening carefully for noises and complained that ordinary everyday noises from outside were 'magnified tremendously'.

Whenever engaged in any act involving

movement, such as dressing, he insisted on having absolute silence and complained that noises prevented him from carrying out the act. He would not give his parents any satisfactory answer to their anxious queries concerning his behaviour and he rejected strongly any suggestion that he was ill and also vigorously resisted the suggestion that he should have treatment.

Mental state on admission to hospital

The patient was well informed and of high intelligence. He had great difficulty in organizing his thinking and talked wide of the point. He often hesitated in the middle of a sentence which he then repeated in a mechanical fashion. Thought blocking occurred frequently. His thinking was of a noticeably concrete type and he gave occasional examples of metonymy and animistic thinking. Abstract thinking was impaired but chiefly in relation to his own bodily processes. He could still abstract and generalize in relation to external events perceived. His thought content was taken up exclusively by his difficulties in moving. His affect was blunted and there was evidence of incongruity. In the main, any affective changes from this base-line of indifference were noticeably related to his success or failure to perform some motor act. A mild paranoid affect emerged if he were pressed to move when he felt unable to do so. He had no insight and rejected the idea that he was ill, persisting in the view that his behaviour was justifiable and could be logically explained.

Transference phenomena

This patient was seen three times a week in psychotherapeutic sessions. He was particularly co-operative, giving an excellent account of the subjective experiences which interfered with his movements. After two to three interviews there was a dramatic change in his general behaviour. He was no longer preoccupied with his symptoms and he was able to move about freely. For example, he ran up the ward to meet the therapist and

thought-blocking had disappeared. Writing was the first spontaneous activity in which he engaged. This was considered to be significant because, apart from sitting and walking, the first motor act which the patient had perceived in the course of his psychotherapeutic interviews was the therapist writing.

The psychotherapeutic situation was complicated by the fact that insulin coma therapy was recommended as the treatment of choice. He was reluctant to follow this advice and only after much explanation was he agreeable to undertaking this treatment in addition to the psychotherapy. Almost immediately the repercussions of this step became apparent in the psychotherapy. He was no longer on time for interviews and there was a general slowing up in movement. He talked less and thought-blocking was once again clearly in evidence. For the first time a persecutory note was noticeable in his communications. He was very suspicious of any suggestions regarding his treatment or management. He constantly asked for explanations of what was happening to him, and at interviews he was preoccupied with the possible effects of insulin on his brain and body. As the insulin treatment proceeded his motility became extremely disordered. He would take 30 minutes to walk 20 yards. Stereotypies of movement (e.g. shrugging of his shoulders) appeared and gradually increased in number, variety and frequency. During this phase the therapist attempted to deal with this relapse by showing the patient that he was unable to express his anger and resentment against him for permitting the insulin treatment. Such efforts had little effect and during the whole course of the insulin treatment the patient remained in the same state. He interpreted casual actions on the therapist's part as persecutory. When asked, for example, to catch a small box (to test his motility) he expressed fear and said: 'What was I talking about before you attacked me with that box?' At the end of the interview he handed the box back saying 'here is your weapon back again'. The projective defence arising from negative transference which he

utilized at this point, and more frequently later, was interpreted to him.

It was reported from the insulin department that the patient's behaviour was catatonic. In the morning, prior to the insulin injection, he would take an hour or more to get into bed, either standing rigidly or engaged in some stereotypy of movement. Long after recovery from coma he would remain motionless in bed, insensitive to his environment. It was at this time that the patient expressed fears of being injured while he was unconscious. He asked the therapist if he was the one who had punched him in the face while he was in coma. He had several dreams of men taking him away in a van and he was afraid that he might wake in someone else's bed. This negativistic behaviour continued throughout the period of the insulin treatment.

The transference was further complicated by the departure of the therapist on holiday. The patient clearly resented this and was enabled to express jealousy and resentment. He eventually asked for permission to leave hospital himself for an identical period of time. On several occasions he was so late for his interview that he was sought out in the ward and during the following interview he spent most of the time talking about a fellow patient who kept pestering him. 'He is always following me about and says that people are persecuting him... however hard I try I cannot dodge him.' This and similar material could be connected with his dislike of anyone being behind him and his difficulty in moving. He had the phantasy that he would have little difficulty in moving if a bull was chasing him. In such an instance the fear of what was behind would be greater than the fear of what lay ahead. He had a similar phantasy whose content referred to escaping from a burning house. His fear of sexual contact in the treatment situation was evident enough in these associations. His transference anxieties were made manifest and this led to the ventilation of both fears and angry thoughts. He asked the therapist if he would allow himself to be blindfolded and led along Sauchiehall Street by a dog.

After the insulin treatment was completed the patient became freer in his movements and went out for walks frequently. That this improvement was dependent upon positive transference was confirmed by a relapse when the patient discovered that the therapist had another male patient in intensive psychotherapy. The day following a chance observation of this other patient leaving the consulting room, he remained in his room in a catatonic posture refusing to talk, move or sit. It was reported that the next day he took up his stance at the consulting room door and assaulted a female patient who attempted to enter the room. A dramatic improvement occurred when the other treatment patient was discharged a few weeks later. Analysis of these manifestations prevented a later relapse when the therapist undertook further psychotherapeutic commitments which the patient came to know about.

Disturbance of visual perception and motility

Movement by the patient was associated with visual perceptual distortion of the environment which he described at various times as 'a flatness', 'a flat streak of colour', 'a painting', 'a wall', 'a canvas', 'a piece of stage scenery'.

I will try and explain again what happens when I stop. I see things in a different way from other people. It is sudden changes that make the difference. When I'm on one side of a corner then I will know when I turn the corner the picture will be entirely different. There is a sudden change in the picture. In a way I perceive things differently. You know those pictures where you look through stereoscopic glasses and you see it in colour and depth but without the glass it seems quite flat and blurred? Well, even if I were wearing the glasses it would still be flat. When I am outside I see everything as just a flat streak of colour. It is just like a painting and I have got to pay attention to each particular part of it. Also my range of vision is different from other people's. It is not so wide. Things that are happening to the side I don't seem to see. If I close one eye it doesn't seem to make much difference. My vision with two eyes seems to be just as narrow.

Moving faster increased the severity of this disorder. 'Moving is like a motion picture. If you move the picture in front of you changes. The rate of change in the picture depends on the speed of walking.'

By stopping all movement and taking much longer to perceive objects visually the patient found that he could cancel the visual disturbance. 'I have got to slow down to see! Stopping obviates the flatness. If you keep going the flatness tends to continue. I stop to obviate the flatness because I know if I continued, the flatness will develop. You only see a still picture if you don't move your head and eyes.'

Sudden changes in visual perception, brought about by sudden alteration in his environment, always induced catatonic-like phenomena. Thought blocking also occurred whenever the patient moved his head and eyes to view some new object which was previously outside his visual field. 'It's the sudden change in what you see that matters. If a tree suddenly sprouts up in front of you, you would stop. I certainly do! Well, the sudden changes at corners are like that. I am incapable of coping with a sudden complete change. I know beforehand that when I get to a corner I will be in a panic. I know what causes it. It's the sudden change of direction.'

If visual perceptions remained constant the patient was able to move freely. He could walk without much difficulty along a long straight road with a fixed distant horizon. He could therefore move more easily outside in spacious surroundings, than he could in the complex environment of the ward where he tended to stand still. The patient could only walk in stages. After proceeding a few yards he became rigid and motionless while he visually fixated the scene immediately in front of him. This accomplished he would walk on a little further. It was noted, however, that he could walk freely if preceded by someone else. 'If I am following someone I pause less frequently because the leader is in a sense "blazing the trail".'

When engaged in the visual fixation of the

environment the patient was unable to attend to auditory or extraneous stimuli or to perform any other motor act, including speech. 'It amounts to this; I can't move and hear things or use my eyes, or talk at the same time. I can't move and do something else at the same time.' The inability to attend to auditory stimuli during visual perception led to what appeared to be a neglect of verbal communications. This neglect was striking in the psychotherapeutic sessions. If the therapist simultaneously moved and talked, the patient's attention was fully taken up with the observed movement. In the subsequent interview he would explain at length that the therapist's movements had occupied his mind to the exclusion of anything that might have been said. This phenomenon occurred with great frequency. Fluent conversation would often be suddenly interrupted as soon as the patient observed even minor movements of the therapist, such as casually tapping his knee with his fingers. On the occasion mentioned above, he lost all interest in the previous conversation and when asked what he was thinking he said 'I was thinking of the mechanics of moving. I was distracted by you moving your hand. It stole my attention and I began to think of movement.'

The patient often performed spontaneous movements of various kinds in relation to visual perception of movement around him. If someone stepped forward he always moved the same distance backwards. If the therapist stepped to the left side, with the patient facing him, the latter always stepped the same distance to the right. If one stretched out a hand towards his shoulder he spontaneously rotated his whole body in an axis, in a direction away from the oncoming object. Similar movements occurred in a circular direction. On one occasion, while standing in the consulting room, he visually fixed a person walking past the window. He made a spontaneous rotary movement, keeping his head, body and eyes in the same axis, in the same direction as the moving figure. When the figure disappeared from view he swung backwards, in the same axis, to his original position. These spontaneous

movements were always made in a direction which would tend to keep the original visual field constant.

On another occasion he was observed standing rigidly outside, fixing the scene in front of him. He appeared to be desperately struggling to move off but seemed unable to do so, and during this time it was noted that he made numerous and varied writhing movements of his head, face and trunk. As soon as he started walking, these muscular writhing movements disappeared.

Disturbance of auditory perception and motility

When the background of silence, or of continuous uniform noise was broken by a new sound or a sudden alteration in the sounds already heard, the patient exhibited catatonic-like behaviour and was unable to move. 'But it's not just seeing into things that prevents me from moving. That's only part of it. If there is a noise, that keeps me from moving.'

'I can't move if I'm distracted by too much noise. I can't help stopping to listen. That's what happens when I'm lying in bed, if there is too much noise I can't move.'

It was observed that the duration of motor blocking was related to the duration of the new sound. Prolonged noise in the ward from the radio or television sets, or from a number of people talking loudly together, rendered the patient completely immobile. The patient had much less control over these phenomena associated with auditory perception, than he had with similar effects produced by sudden changes in the visual sphere. 'I can do something about what I see. For example I could turn round and look at this blank wall. But I can't do anything about sounds. It's much more difficult. I've got to listen until it stops. That's when I pause.'

Whenever the patient made a voluntary movement, such as putting a sweet in his mouth, a sound occurring while the movement was still in progress interrupted the motor performance. On one such occasion the therapist spoke, and the patient's arm became

fixed halfway to his mouth. He appeared startled and said 'You frightened me just now. I was giving all my attention to putting a sweet in my mouth when you spoke. You stole all my attention, then I did not know what my hand was doing. It was out of my control.'

Disturbance of body image and motility

In order to be aware of the position of his body in space the patient required to devote his entire attention to this process, and had to neglect all extraneous stimuli. 'It's very difficult to know in a fraction of time what the actual position of your body is. If you are standing with your legs in corresponding positions, say standing at attention, it's easier to get aware of your actual position. Lying in bed is easier still. If the position is more complicated you've got more detail to remember. It takes longer to find out. You just made yourself aware of it by thinking about it.'

He was chiefly concerned about the position of his feet and hands. He would often move his trunk while keeping his feet fast to the ground and this was particularly noticeable if anyone tried to give him a push. He seldom took his hands out of his pockets, and rather than push open or shut doors with his hands, he used his shoulder.

I don't have to think about my fingers, at least not usually, because they go with my hands. It's chiefly my arms and legs that matter. If you know where they are, you can tell what your position is.

Although you are one integral thing, there are certain things you can do without. For example an amputated leg. You can remove some part of you and you still remain yourself. My body is not quite separate but not quite integral either.

You can separate something but it doesn't affect your thoughts. So obviously you can detach yourself from certain parts of your body and still be yourself. That is connected with wondering where my hands are. Or, if I have difficulty in moving my legs, it's something like being detached from them... 'You see where your hands are now doctor? Well just keep them still. Now you can't remember putting them there can you? Well, you did it unconsciously didn't

you? And in a few minutes your hands will be somewhere else. Well, that means that your hands can move about without your knowing it. Now of course I know that my hands are at the ends of my arms. You see you might put your arms subconsciously round the arm of that chair, but if I did that I would have to take stock and ask myself 'where have I put my hands now?'

If the patient did not have his hands in his pockets then he invariably carried some object, often unusual or inappropriate, in his hand, which he could see and feel. The variety of such small objects was very large, from a piece of string to an electric razor.

In order to carry out any movement the patient had to give all his attention to thinking ahead, visualizing how he would perform each part of the movement. He explained that by thinking he obtained a mental picture of his body position, and then had to visualize his body in the new position before he could move into that position. He appeared to be able to move only after conscious reference to a series of mental pictures of his body, treated separately and concretely. Sometimes he found it difficult to shift his attention from one such position to another, and when this happened he found considerable difficulty in getting out of whatever position he was in.

If you are going to move from one position into a new position, there's a motion picture in between. It's got to be a still picture. Supposing while you are moving I said 'Stop'. Then you couldn't think of the final picture. You would be stuck! I have to see into each picture before I can move into it. If I start doing something, then start thinking of what I'm doing, that locks me up in a sense. For example if I drop something and then stoop to pick it up, if I start to think of myself in that position and what I'm doing, that locks me up in that position. If you keep thinking of where your body is, it gets locked up. You can't think of your body in one position and think of it in another position at the same time.

The performance of any movement dis-regarding this process of attending to the motor act, aroused in the patient intense fear, the effects of which could be observed, and also

the subjective sensations of unreality. An example has been given above in connexion with putting a sweet in his mouth. If he so moved without carrying out his usual procedure, he feared that his body would get into bizarre or unusual positions. 'I am afraid to move without giving all my attention to it because if I am doing something else, I might carry out the wrong movement. For instance on going out of that door if I paid attention to something else I might stand on my head.'

Disturbances in communication

It was repeatedly observed, as described above, that the patient neglected to attend to the content of the therapist's spoken words if the latter moved, if the patient himself moved, or if new auditory stimuli intervened, no matter how minor any such changes may have been. 'A movement in front of my eyes is like hearing a sound in my ears. You can have your ears open and hear nothing and you can have your eyes open and see nothing.' Verbal communication was seldom effective because these sources of disturbance, which were usually related to ordinary and even trivial events, could rarely be eliminated completely at any given time. Initially the patient could not describe his difficulties in moving. Later it was recognized that he could only express these subjective experiences whenever he perceived the movements of someone else. For this purpose the therapist was the most convenient model.

'It would be easier if we talked of a notional third person. I could more easily understand if we described this illness on a third person. Then I could understand it. I can't understand it when it is applied to myself.' Thus it was found that the patient could talk more freely of his internal mental events if these were externalized and projected away from himself onto the therapist. As referred to above, he rarely initiated a discussion of his difficulties. Once started by means of the visual cue he might spontaneously say something more directly about himself. Numerous illustrations have been given above of how the patient's thought content was influenced by movements

of the therapist. The latter made his main impact upon the patient through his actions. Communication was achieved by movement rather than by words. This applied particularly to all bodily movements. It was noted the patient carried out movements only after he had first perceived the therapist performing them. He behaved similarly with any psychological test involving motor action, such as the Rorschach.

COMMENTS ON THE CLINICAL MATERIAL

As in every psychotherapy the patient revealed manifestations of transference and they have been detailed above. Certain of these manifestations may be interpreted as the consequence of homosexual conflicts. Unfortunately the same phenomena could be thought of as a defence against destructive tendencies. The concept of projective identification whereby the patient puts sadistic phantasies into the therapist might also be employed to explain some of these transference manifestations. The patient's relapse after the initial improvement and the re-appearance of catatonic reactions on a number of occasions provide good examples of this. Is this, however, the true explanation? While immediate acknowledgement must be made of the influence of affective tendencies either to improve or retard the pathological process this does not necessarily mean that the symptoms symbolize a series of unconscious conflicts, as in the case of the psychoneuroses. The question must be—can such conflicts as represented by the type of interpretation referred to occur within the psychotic part of the patient's mind? The authors believe that the clinical phenomena of this case permit a possible answer to this question.

The disturbance in the patient's mind affected the spheres of motility and perception. The abnormality of these functions in themselves were sufficient to account for the patient's symptoms and behavioural difficulties. Many of the phenomena which might be considered as transference were the result of these abnormalities—for example, the patient's failure to respond to verbal com-

munications and his general tendency towards negativism. This failure, as the authors came to learn, resulted from the patient's anomalous reactions to all extraneous stimuli. As he put it: 'You can have your ears open and hear nothing.' His customary reaction was more in the nature of a primitive form of identification based upon visual perception. If the therapist moved the patient was completely oblivious to whatever was said and his immediate, manifest reaction, when it occurred, appeared in the sphere of motility. Only later was he able to describe what he had experienced. In the authors' opinion many of the phenomena which were observed during psychotherapy arose from the psychotic part of the patient's mind—particularly those which could be described as catatonic. Although they could be described as transference manifestations it seems important to distinguish them from the *Transference Phenomena* described earlier under a separate heading; which are of a different order; which spring from the non-psychotic layer; and of which the content and source of origin are no different from that encountered in patients suffering from psychoneuroses. Indeed, the material elicited from this patient indicates that the employment of the concept of defence as it is found in the psychopathology of the neuroses would be misleading and superfluous for phenomena which are so plainly the outcome of primary disturbances of ego functions; and to seek alternative hypotheses is to deny the value of the phenomena themselves.

It was particularly obvious that the patient's affective reactions were the result of the basic defect in motility and perception. Such reactions did not precede the failure of these functions but consistently followed them. He became fearful whenever the necessity to move arose. He referred to such subjective experiences as 'panics', 'emergencies', or 'being in a spot'. When he was unable to move, as for example being unable to get out of bed, he became quite depressed. If he was pressed to move while he himself was struggling to do so he became hostile and expressed ideas of a

persecutory kind. This reaction became comprehensible in terms of the necessity which he felt, prior to movement, to fixate the image of each succeeding body movement and also some particular detail in his visual field.

DISCUSSION

The authors subscribe to the hypothesis that in schizophrenia a regressive process affects the mental organization which Freud (1923) described as the ego and that it is the locus of the pathological disturbance. In the patient described above the regression principally involved the functions of motility and perception, thus leading to the symptoms and signs of the disease. His catatonic reactions in response to a variety of extraneous stimuli, particularly visual and auditory, can be compared with somewhat similar phenomena observed during experiments conducted by Werner & Wapner (1952) in the psychological laboratory. They base their concept of 'functional equivalence' upon experimental findings in which sensory (auditory) and tonic (muscular) stimulation led to an identical perceptual change. For example, an electrical stimulus applied to the neck muscles or an auditory stimulus results in the vertical being seen as tilted towards the side opposite to that of stimulation. Clinically the patient's motor (catatonic) reaction was identical irrespective of the nature of the perceptual stimulus. In the experimental situation there was an identical perceptual response to different forms of stimulation. Other experiments (Werner & Wapner, 1952), in which auditory and visual stimuli were employed, also showed that the extraneous stimulation of the organism in one sense-modality produced a direct response in another sense-modality. Further experimental study suggests that this functional equivalence is most pronounced in the case of the young child who is still functioning with a relatively undifferentiated psychophysical organization. The schizophrenic demonstrates the same diffuseness in his reactions. Functional equivalence was present in the patient's inability to differentiate visual from auditory percepts—

'a movement in front of my eyes is like hearing a sound in my ears'.

The patient gave the impression, both from his utterances and his behaviour, that he was wholly taken up with achieving a degree of stability in his visual perception. When this was obtained he became mobile. The loss of the stability of perceptual experience in schizophrenia and its effect upon other ego functions has been observed by the authors (1957) and by Sechahaye (1956). A female catatonic patient (Freeman & McGhie, 1957) declared that she did not move 'because things won't stay still and I am afraid'. A more lucid account of the same phenomenon is contained in the following remarks by a catatonic patient reported by Sechahaye (1956): 'All the objects were transformed. I no longer recognized them. They suddenly took on other forms and I lose the only stability I had. . . I did not want to move, because if I did everything changed around me and upset me horribly so I remained still to hold on to a sense of permanence.'

As in these cases the patient obtained stability in visual perception by fixing his head and eyes upon some object. Piaget (1951) has pointed out that the 6-month-old infant who wants to follow the movements of a person he is watching must make the same movements himself. In Piagetian terms the infant can retain his perception of movements by accommodating to them. Perception is subjectively orientated at this stage. The spatio-temporal order of adult perception is absent, the slightest change in the perceptual field resulting in the whole structure of that field being altered. Thus, to the infant a movement of the head is enough to change the characteristics of all objects perceived.

The patient's response to forcible attempts to make him move have already been referred to. His panic-reaction can be explained as the result of the fear of losing the perceptual stability which he had momentarily obtained. Mobility was possible whenever visual perceptual stability was ensured. When this condition no longer obtained the patient had to stop in order to regain the lost perceptual stability.

If he failed to stop he experienced anxiety and sensations of unreality. Experiences such as this provide formidable support for A. Freud's (1952) theory that negativistic states may be the result of an attempt to stave off a threat to the integrity of the ego. She points out that it is the regression to primary identification with the love object which is feared—'this is a regressive step which implies a threat to the intactness of the ego, i.e. a loss of personal characteristics which are merged with the characteristics of the love object. The individual fears this regression in terms of the dissolution of the personality, loss of sanity, and defends himself against it by a complete rejection of all objects (negativism)' (A. Freud, 1952).

The clinical observations which were obtained during psychotherapy indicated that the pathological process within the ego leads to a state in which ego functions which are normally discrete and differentiated interact with one another and lead to frightening and unusual experiences. In the attempt to dispel such sensations the patient initiates behaviour which is described as catatonic. If the concept of defence has a place in the psychopathology of psychosis it is in this context. The fear of ego disintegration and loss of identity stimulate activities whose aim is to restore the unity of the personality. These activities are the clinical manifestations of the psychosis. Hartmann (1953) has advanced similar ideas in the suggestion that defences 'on a lower level of integration' are utilized to deal with the after effects of ego damage. This concept of a reactive defence which follows upon the breakdown of the ego is akin to Federn's (1948) theory that defence is never a primary manifestation in psychosis but represents a means of dealing with the results of the breakdown of the ego.

The theory of schizophrenia described above has important implications for the psychotherapy of the condition. It reveals that communication between the patient and the therapist is qualitatively different in schizophrenia from the psychotherapy of the neuroses. In the latter verbal communication in the form of interpretations plays the decisive role. This

is not so in the psychotherapy of psychoses. The patient's difficulty in perceiving the spoken word was only one aspect of the effect of the disease process upon his ego functions. Interpretations which would be correct in a case of psychoneurosis become quite inappropriate. For example, the patient's loss of mobility was not modelled along the lines of a psychoneurotic symptom. It was not a defence against unconscious phantasy but was due to the intricate conditions which ensured some stability for the patient's ego. Similar considerations applied to his periodic inability to speak. Schilder (1951) has pointed out that when schizophrenic patients stop speaking it is not always the result of a preoccupation with conscious phantasy. There is the possibility that the thought blocking occurs because motor impulses are not at the patient's disposal. This explanation appears to be applicable to the patient under discussion. When dominated by the urge to move or when overwhelmed by auditory or other percepts he found himself without the necessary motor power to activate his speech apparatus.

According to the approach adopted here psychotherapy in psychotic reactions must be primarily concerned with re-establishing the integrity of the ego. In this aim the authors follow the technique of treatment recommended by Federn (1948), the details of which need not be gone into here. Federn (1948) demonstrated on many occasions that the first delusions which substitute for actual events and the first falsifications which replace a correct apprehension of reality can be attributed to occurrences of the previous day in the patient's life. These events occur long before there is an actual break with reality. The establishment of a positive relationship to the therapist is a pre-condition for the correction of these distortions brought about by ego regression. Once a positive transference is established the patient can be induced to undertake the task of examining the unhealthy part of his ego. This was the method employed in the present instance. In successful cases the patient becomes aware of the means whereby

he has falsified reality. He comes to recognize the difference between those subjective experiences—whether in the sphere of thought, perception or motility—which spring from the healthy part of his mind and those which arise from the regressed ego functions.

The purpose of this paper has been to show

how in the course of psychotherapy a wealth of clinical material emerges which can be related to concepts derived from psychoanalysis and from experimental and genetic psychology. Such an approach holds out possibilities for a better understanding of the signs and symptoms of schizophrenia.

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ON ASSESSING THE RESULTS OF PSYCHOTHERAPY*

By DAVID MALAN

Psychotherapists are faced with the extraordinary situation of being unable to provide conclusive evidence that they do any useful work at all. This, I believe, is largely because our increasing knowledge of psychodynamics has never been fully used in the assessment of results. The present paper, starting as an attempt to fill this deficiency, leads to a line of research which may perhaps provide the evidence needed.

In a physical illness we can assess a therapeutic result from the changes in symptoms and signs, but we cannot do this accurately without an hypothesis about the underlying pathological anatomy or physiology. My essential thesis is that the same applies to neurotic illness, but that here the requirement is an hypothesis relating to the pathological psychodynamics. The main obstacle has always been the apparently arbitrary and far-fetched nature of psychodynamic theory. The present paper is part of an attempt to overcome this difficulty by as strict a discipline as possible in the amount of theory permitted. The basic generalizations of psychodynamics are assumed, e.g. the part played in neurotic illness by unconscious conflict and defence mechanisms; but in forming a psychodynamic hypothesis about any individual patient, particular attention is paid to (a) setting down the facts on which the hypothesis is to be based, and (b) not permitting the hypothesis to go beyond that minimum by which the facts can be causally connected. If these rules are observed I believe that psychodynamic processes, intangible as they are, can be treated in a much more rigorous and acceptable way than has hitherto been attempted. A solution to this problem should lead to a more accurate method of assessing psychotherapeutic results.

This paper starts with a review of previous

methods of assessment which can roughly be divided into two classes: *subjective* methods, e.g. those of the 'Much improved', 'Improved', 'No change' type; and *objective* methods, e.g. those based on a quantitative treatment of the patient's answers to a questionnaire. This is followed by an account of those comparisons which have been made, using these methods, between improvements observed in treated and those observed in untreated patients. The results of these comparisons are, to say the least, equivocal. Any method of assessment must, however, do justice to very great quantitative differences, and important qualitative ones, between one therapeutic result and another. Evidence is brought forward to suggest that the 'subjective' methods of assessment hitherto used fail to do justice to the quantitative differences; and the 'objective' methods to the qualitative. It is suggested that this largely vitiates previous methods of assessment and constitutes one of the main reasons why the controversy about the value of psychotherapy remains so far from being settled.

The main qualitative difference between results is the distinction between 'resolution' of a neurotic conflict and various kinds of 'false' or apparent solution, of which 'flight into health' is an example. This distinction can only be made by means of a psychodynamic hypothesis about the patient's illness. Since no one patient is exactly like another, no method of assessment based on general criteria is specific enough to give accurate results, and it is necessary to make a separate hypothesis in each individual case. A technique has therefore been developed of forming a minimum psychodynamic hypothesis which can link together the various disturbances in a given patient. With the aid of such an hypothesis, criteria can be laid down before therapy, criteria which must be fulfilled if the result is

* Manuscript received 17 November 1958

to be regarded as satisfactory. Examples are given of the use of this method with patients treated by members of Dr Balint's Workshop for Brief Psychotherapy at the Tavistock Clinic, London.

In a final section the practical consequences are considered. Most important of all, the arguments presented here lead, I believe, to a partial solution of the problem of providing 'controls'—without which no study of the results of psychotherapy can be of much value.

PREVIOUS WORK

A. 'Subjective' methods

An account of methods of assessment used by previous authors, more or less in increasing order of their refinement, will serve to introduce the difficulties one by one and to show how far they have been overcome in the past.

The simplest method is a 3-point scale (e.g. 'Recovered, Much Improved, No Change'), without any statement of the criteria on which the scale is based, and without examples which might provide an idea of the criteria used (Ernest Jones, 1936; Maudsley Hospital Medical Superintendent's Report, 1931).

Other studies overcome some of the obvious objections by giving criteria and examples. Neustatter (1935), for instance, used a slightly more elaborate scale, and defined his criteria in terms of symptoms—e.g. in the class 'Moderately Improved', symptoms had shown a 'satisfactory diminution in intensity or frequency' but had not disappeared. Two main objections to this are by now well recognized. These arise from the dynamic conception of neurosis, and represent some of the fundamental difficulties in this whole field. The first is that a symptom may disappear for a number of different reasons without any solution of the 'basic' problem, e.g. it may disappear as a response to relief of external stress, to the relation with the therapist ('transference cure'), or as a 'flight into health'. The problem arises of deciding which factor is operative.

The answer can to some extent be given by long follow-up (Hamilton & Wall, 1942).

Obviously the longer a 'cure' lasts, the more likely it is to be due to a fundamental change. Even a long follow-up, however, fails to meet the second objection. This is that the disappearance of the particular symptom may be balanced by some other impoverishment of life which is not so obvious but equally crippling. The clearest example is that of a patient who breaks down under some situation which is specially stressful for him (e.g. the temptations of heterosexuality), and can allow himself to lose his symptoms only by arranging his life so that he avoids such a situation in future.

Several authors do in fact make clear that they are aware of this fallacy, and stipulate that the patient must have been able to meet situations of stress without relapse (e.g. Luff & Garrod, 1935; Rosenbaum, Friedlander & Kaplan, 1956). Similarly, Ripley, Wolf & Wolff (1948) distinguish in psychosomatic cases between 'symptomatic' and 'basic' improvement, stating that the latter depends on the patients' 'having encountered a major threat in their life situation and having shown a reorientation in outlook by meeting it in a more constructive way and without symptoms'. A possible object to this is that the 'major threat' is not necessarily the specific threat for that particular patient.

The question now arises of how it may be possible to distinguish between 'symptomatic' and 'basic' changes *before* the end of a long follow-up—though the long follow-up is of course always necessary. The obvious answer seems to be that one must be aware of what the patient's 'basic' difficulties are, and that changes in these difficulties must be considered in assessing the result. This introduces an important problem, since objectively verifiable changes are not necessarily correlated with 'basic' changes; and basic changes may be the least objectively verifiable of all. This objection, for instance, applies to any study based on the criterion of discharge from hospital (e.g. Landis, 1938); or on the ability to work—one of the criteria used by Harris (1954) in a comparison of psychotherapy with CO₂ abreaction.

Many authors have shown that they are

fully aware of this difficulty. Seitz (1953), who studied twenty-five cases of 'psycho-cutaneous excoriation syndrome', observed that even though the symptom itself disappeared after the abreaction of anger, there was a strong tendency for the patient to re-establish masochistic reactions to anger-provoking situations after the treatment was over. Some of the studies of the results of psychoanalysis are also clearly concerned with 'basic' rather than 'symptomatic' changes. For instance, Kessel & Hyman (1933) and Hyman (1936) performed a follow-up of forty-three patients whom they had referred for psychoanalysis. These authors introduce the concept of a 'specific' satisfactory result, one which could not have been brought about by any other method. Such a specific result is, for example, the cure of a male patient's sexual perversion. The authors provide examples but give no general criteria by which such results may be recognized.

A more extensive study, also with strict criteria, was made by Alexander (1937) on 226 patients treated at the Chicago Institute for Psychoanalysis. He used a 5-point scale from 'Apparently cured' to 'Aggravated', and stated that his first two categories were used most conservatively: 'They do not refer to patients who were merely freed from their symptoms, but a substantial improvement of the pathological condition was required.' Moreover, one of Alexander's important contributions was to specify the degree of change in more than one area of the patient's illness, e.g.:

Neurotic depression	Apparently cured
Hysterical anxiety state	Unchanged
Character disturbance (infantile personality)	Unchanged

This method has been greatly refined by other workers who have considered the patient's adjustment in a number of different areas before and after treatment. Knight (1941) lists five such areas—symptoms, productivity, sexual adjustment, interpersonal relations, insight—and makes an overall assessment on a 4-point scale. Miles, Barrabee &

Finesinger (1951) refined this method by rating the patient in each of five areas of adjustment on a 5-point scale, with carefully defined criteria, and combining these on a 6-point scale. The most detailed method of all is that proposed by Morse (1953), who surveys six areas of the patient's life, each subdivided many times, and uses a 4-point scale in each of the forty subdivisions. Morse has also taken the necessary step of checking the reliability of this scale by showing that five different assessors are able to achieve a good agreement on any single case.

B. 'Objective' methods

A disadvantage of most of the methods already described is that the final assessment has to be based on a rather vague estimate of 'improvement'. In the last 10 years there has been an extensive search for variables which can actually be measured or counted. Much of this work has been carried out in America by the 'Client-centred' therapists led by Carl Roger.

These methods can be divided into three main classes:

(1) Projection tests

(a) *Rorschach*. This is a test for which reliable quantitative methods of scoring have been devised, but unfortunately recent work has cast considerable doubt on their validity as measures of improvement. Thus Carr (1949) found no significant change in the scores of nine patients treated by 'Client-centred' therapy for an average of six sessions, and rated as improved by other criteria. Barron (1953) found it of no use in predicting those patients likely to improve in 'dynamically oriented psychotherapy'—there was no correlation between improvement and any of the conventionally used scores or ratios found in pre-therapy tests.

(b) *Thematic apperception test*. This is a test which is usually interpreted qualitatively. Grummon and John (in Rogers & Dymond, 1954) have developed an elaborate method of making interpretations more reliable and

quantitative. Twenty-five different factors (e.g. attitude to mother, mate, children, work, social activity, etc.) are rated, each on a 7-point scale. Elaborate criteria are given for each rating (e.g. mother is viewed in an acceptant fashion, as a sexual object, as someone who desires to possess the patient, etc.). The scales are averaged to give an overall rating on a 7-point 'scale of adjustment'.

(2) *Methods based on questionnaires*

(a) *The Minnesota Multiphasic Personality Inventory* (Hathaway & McKinley, 1940, 1943). This consists of a total of 550 statements which the patient is asked to sort into 'true', 'false' and 'cannot say'. It has been extensively studied and validated by being given to 'normal' people and to patients with unequivocal psychiatric diagnoses, and a number of scales have been derived from it (e.g. hypochondriasis, depressive). It was originally intended for diagnosis but has recently been used in assessing the results of treatment.

(b) *The Q-sort*. This was developed by Butler and Haigh (in Rogers & Dymond, 1954), following the methods of Stephenson (1953). The patient is given 100 cards on each of which is a statement. He is asked to sort the cards into nine piles from 'least like me' to 'most like me', and is told how many cards to put in each pile so that a 'normal distribution' is assured. The most effective use of this test can be made by asking the patient to carry out the same operation for his 'ideal self', and then calculating the correlation between the two. This is known as the 'self-ideal correlation'.

(3) *Quantitative analysis of statements made by the patient during therapy*

This has become possible since the introduction of tape-recording. Seeman (1949) rated the patient's statements as 'positive' or 'negative' (with a 76 % reliability between different judges), and showed a significant increase in 'positive' and decrease in 'negative' statements during 'client-centred' therapy.

COMPARISON OF IMPROVEMENT IN TREATED AND UNTREATED PATIENTS

Eysenck (1952) has reviewed previously published figures on the improvements observed in patients treated by psychotherapy of all kinds. These included nineteen studies of eclectic psychotherapy and five of psychoanalysis. The scales and criteria of assessment used varied over almost the whole range of the 'subjective' methods listed in the previous section. Eysenck reduced all these scales, in effect, to a dichotomous scale of 'Recovered, Much Improved, or Improved' on the one hand and 'Slightly Improved, No Change, or Worse' on the other. The overall proportion of 'Recovered or Improved' patients was 64 %.

Up to the time of publication of Eysenck's paper there had been no studies of changes in genuinely untreated patients with which this figure could be compared. He therefore adopted the reasonable compromise of regarding psychotherapy as 'specific' and comparing it with 'non-specific' methods. Two large-scale studies of such methods had been published at that time, one by Landis (1938) on patients treated by a hospital regime with only limited psychotherapy; the other by Denker (1946) on patients treated by general practitioners.

Landis found that 72 % of all patients admitted to the New York State Hospitals for psychoneurosis between 1917 and 1934 were discharged as 'recovered' or 'improved'. Denker studied 500 consecutive disability claims in the files of an insurance company, in which the patient had been out of work because of psychoneurosis for at least 3 months, and had received treatment from general practitioners only. The patients were followed up through their general practitioners for 5-10 years. Criteria of 'recovery' were (a) ability to carry on at work for at least 5 years, (b) complaint of at most very slight difficulties, and (c) successful social adjustments. The results were that 45 % of patients 'recovered' after 1 year, and another

27 % after 2 years, making a total of 72 % once more.

Eysenck concluded: 'The data...fail to show that psychotherapy, Freudian or otherwise, facilitates the recovery of neurotic patients. They show that roughly two-thirds of a group of neurotic patients will recover or improve to a marked extent within about two years of the onset of their illness, whether they are treated by means of psychotherapy or not.'

The main criticism of this conclusion, which has been given by many authors, and which I shall elaborate later, is that the figures for 'Recovered or Improved' in different studies are not comparable, because of great differences in the criteria of assessment used. There is clearly a great need for properly controlled studies, making use of genuinely untreated patients, who can be compared with treated patients by the same investigators using the same criteria.

Since the publication of Eysenck's paper there have been at least three such studies. The first is by Rogers & Dymond (1954). These authors report on the results of 'client-centred' therapy with adult patients, combining quantitative 'objective' methods of assessment with a more rigorous use of the older 'subjective' methods. Therapy was given to twenty-nine patients for an average of thirty-one sessions, over an average total period of 33 weeks.

The twenty-nine treated patients were divided into two roughly equal groups, one of which ('Experimentals') was given treatment at once, and the other ('Own Controls') had to wait 60 days. There was a third group of twenty-three volunteers with no need for therapy, who constituted the 'Controls'.

The main methods of assessment were as follows:

(1) 'Counsellor Rating'

The Counsellor was asked to rate the personal integration and life adjustment of the client before and after therapy on a 9-point scale, and to make an overall rating of the outcome of therapy, also on a 9-point scale.

(2) Tests

The main tests given were as follows:

(a) *TAT*. An assessor was given transcriptions of the tests, without being told in what order they had been given to a particular patient (and with all clues removed), and was asked to assess the patient's adjustment on a 7-point scale as shown by each test.

(b) The 'Self-ideal Correlation' test.

The main results were as follows:

(1) The experimental group was significantly lower on the scales before therapy than the control group.

(2) There was a significant increase on the scales in the two treated groups after therapy, and no significant change in the control group.

We can thus say that Rogers & Dymond's study gave a result favourable to psychotherapy. The two studies which follow did not.

Barron & Leary (1955) compared the changes on the M.M.P.I. Scale in 127 treated patients, and in twenty-three patients who during the course of routine clinic work had to wait without treatment for at least 6 months. Treatment was either 'brief ego-oriented individual therapy on a once-a-week basis' (42 patients), or group therapy 'with emphasis on current interpersonal responses' (85 patients), with a minimum of 3 months' treatment in either case. At the end of the experiment there was no significant difference between the two groups.

Teuber & Powers (1953) reported an experiment in which 325 'underprivileged' boys were given individual guidance and counselling over an average period of 4 years, in the hope of preventing delinquency. The counsellors included psychoanalysts and 'client-centred' therapists, and for part of the time worked under a psychoanalytically trained supervisor. The boys were matched with a control group of 325 boys who received no counselling. At the end of the experiment the counsellors believed that they had substantially benefited two-thirds of the treated boys. The final result of the experiment was that ninety-six of the experimental boys made a total of 264 court

appearances for delinquency, as against ninety-two of the control boys who made 218 court appearances. The authors state that but for the controls this would have been published as yet another piece of successful work; and they conclude that 'the burden of proof is on anyone who claims specific results for a given form of therapy'.

A possible objection to the study by Barron & Leary is that the methods of therapy used were probably not powerful enough to effect significant changes in a time as short as 3-6 months. The only reservation which can be made to the study by Teuber & Powers is, on the other hand, that psychotherapy with delinquent children is a very specialized field, and is probably not strictly relevant to work with neurotic adults with which this paper is concerned. Their study does emphasize, however, how necessary it is to check clinical impressions in a rigorous way.

There have been several studies which indicate that the tendency towards spontaneous improvement in neurotic patients is stronger than has hitherto been believed. The most important of these is by Saslow & Peters (1956), in which the method of assessment was by means of the psychoanalytically based multiple 5-point scale of Miles *et al.* (1951), already described. These authors studied 100 consecutive patients who had had consultations at the medical clinics of Washington University, and who fulfilled the following criteria:

(1) They had been diagnosed by Saslow himself as suffering from a 'behaviour disorder'.

(2) They had had no more than two interviews, for various purely routine reasons; e.g. lived too far away, refused treatment, were expected to be able to manage, were thought unlikely to respond, or were simply asked to return for drugs at times of stress.

Eighty-seven patients were located. Follow-up varied from $1\frac{1}{4}$ - $6\frac{3}{4}$ years—in 80 % it was over 4 years. The results were:

'Apparently recovered' to 'Improved'	37 %
'Slightly improved' to 'Worse'	63 %

This may be compared with the figures of 58 % and 42 % respectively, obtained by Miles *et al.* with the same scale on seventy-six patients with a diagnosis of 'anxiety', who had had in-patient analytical psychotherapy and a minimum follow-up of 2 years.

Thus a psychoanalytically based method of assessment also gives a rate of spontaneous improvement which, though lower than those given in previous studies, is still surprisingly high.

This tendency towards spontaneous improvement is confirmed by two long follow-up studies, not of untreated patients, but of patients in whom the prognosis is always supposed to be poor, namely cases of obsessional neurosis. Lewis (1936) located fifty obsessionals who had been treated by various methods at the Maudsley Hospital 5 years or more previously. Sixteen of these patients (32 %) were 'quite well' and had been so for years; seven (14 %) were 'much improved'—a total of 46 %.

Although we may question Lewis's results on the ground that he used purely symptomatic criteria, we cannot do this to the same extent with those of Müller (1953). He located fifty-seven obsessionals who had been treated at the Burghölzli Clinic and the Zürich Poliklinik since 1916. The nature of the treatment is not stated, but we may suppose that it was certainly not psychoanalysis. The follow-up period for all patients was over 5 years, for well over half at least 20 years. The results in his first two categories were as follows: (1) 'socially adapted, able to work, fully efficient, happy family life, free from any obsessional symptoms': 28 %; (2) 'socially adapted, able to work, relatively good human relations, obsessional symptoms still present but clearly much milder': ...21 %; i.e., according to the usual standards, the total of 'apparently recovered' to 'improved' was 49 %.

These results indicate that the tendency towards spontaneous improvement must be taken into account in any assessment of the value of psychotherapy, and emphasize more than ever the need for control studies.

DISCUSSION OF PREVIOUS METHODS OF ASSESSMENT. TOWARDS A METHOD OF ASSESSMENT BASED ON PSYCHODYNAMICS

A. *Quantitative differences between results*

To illustrate these I should like to take two extreme examples:

The first is reported by Rothenberg (1955). The patient was a dentist who had suffered from severe attacks of hiccupping for 3 weeks, after being anaesthetized for incision of an infected finger. He was seen for one session. He said that he was afraid of anaesthesia, whether administering it or receiving it. Rothenberg suggested he was afraid because it blocked control of thought, and he might reveal some guilty secret. The patient confessed that the cause of his infected finger was that he had been bitten by a woman patient, with whom he had had an affair, while performing an extraction on her under anaesthesia. The hiccups immediately stopped and did not return.

The second is reported by Schjelderup (1955). The patient, a woman of 42, had suffered from severe compulsive doubt affecting almost every thought for 21 years, and accompanied by a general feeling of 'dislike' which destroyed all pleasure in life. This had not been improved by marriage, divorce, or various kinds of psychotherapy over a total of 5 years. After 3 years of analysis she was almost free from symptoms, but had relapses lasting a few days during the next 2 years, which necessitated further irregular sessions. Her condition then became stable. Follow-up, 14 years later, revealed that not only was she completely freed from all her symptoms, but (1) her whole personality was more harmonious, (2) her sex life now contained a strong element of tenderness which had not been present before, and (3) the whole world had become more real for her, 'as though a veil of mist had been drawn aside'.

There can be little doubt that the degree of improvement in the second patient is many times as great as that in the first; but whatever scale is used to record the approximate

quantitative estimate of improvement, the actual values given must be arbitrary, and we cannot gain any idea of how much more improved the second patient is than the first; or what the ratio of the second patient's improvement to the first might be.

B. *Qualitative differences between results.*

The psychodynamic assessment of results

The basic qualitative difference between results is that between a 'false solution' and a 'true resolution' of a neurotic conflict. This is a distinction which is implied in the criteria of assessment used by many authors, especially psychoanalysts. It is implied, amongst those already quoted, by Ripley's distinction between 'symptomatic' and 'basic' improvement; by Kessel & Hyman when they speak of 'specific' improvement; and by Alexander when he speaks of 'a substantial improvement in the pathological condition'. Equally it is implied by Fenichel (1930), who lays down that the category 'recovered' must only include 'changes in condition which are completely understandable analytically'. This is the psychodynamic approach to the assessment of results which is the whole theme of the present paper, but the value of this approach is obscured if the results are represented on a simple 3-point scale. The problem is how a better method can be devised.

Before the method can be considered however, a solution is needed to the problem of how to find reliable criteria with which to distinguish between 'symptomatic' and 'basic' changes, or, in my words, between 'false solution' and 'resolution'. This problem, which was touched on earlier, will be discussed in detail now.

Criteria of 'resolution' must depend on a belief in some kind of psychodynamics; but, in accordance with the principle of economy of hypothesis, this belief need not embrace any far-reaching system. This is best illustrated by means of examples.

The first is from our own work: a young man who, while staying alone in an isolated part of

the country, developed a feeling in his anus which gave him the fear that he was homosexual. This lasted for about 3 months. It disappeared when he met a girl with whom he became friendly. One year later this girl left him for an older man, and his symptom immediately returned with redoubled intensity.

It needs only a limited belief in psychic causality to make the following hypothesis seem plausible: (1) that the fact of having a girl friend in some way obscured this man's problem over homosexuality, and (2) that losing the girl exposed this problem once more. If this is so, then although his symptom disappeared when he met this girl, there is strong evidence that his problem over homosexuality remained unsolved. This is an example of 'false solution' by relief of external stress.

The further history of this patient is as follows: He was seen for psychotherapy, during which it emerged that (1) he had helped his mother to divorce his father, about which he felt very guilty; (2) all three of the girl friends that he had had during his life had been fatherless; (3) his homosexual feeling became especially strong when he felt he was not being man enough; (4) he was unable to face competition with other men for a girl.

He was seen for four sessions, after which he decided to go back home in order to start work in his father's business; and when he was seen 3 weeks later, just before he left, his homosexual feeling has almost completely gone.

These facts can all be brought together by a hypothesis based on Freud's concept of the Oedipus complex: (1) that he felt guilty about taking his mother away from his father; (2) that this guilt was carried over into any situation of competition with another man; (3) that his defence against this guilt was to submit himself to men, which could be expressed in a homosexual way.

If this were true, then this therapeutic result must remain suspect, since a possible explanation of the disappearance of his symptom is that his returning home was a way of submitting himself to his father and thus avoiding competition with him

A hypothesis about psychodynamics may therefore enable us to judge an apparent recovery as being 'indistinguishable from a false solution'. But this hypothesis may also enable us to lay down criteria for a 'true solution', which are specific for a particular patient. If it is true that this man's problem is concerned with guilt about rivalry with men, and with his father in particular, then such criteria would be that *in addition to the loss of his symptom* there should be evidence: (1) that he was now able to treat his father as an equal, and (2) that he was able to compete with other men for a girl.

As a second example we may take Schjelderup's case, already quoted, the woman suffering from compulsive doubt. (A much fuller account of the apparently complete 'resolution' of a severe neurosis, with a 5-month and 2-year follow-up, is given by Schjelderup (1936); and he reported a 13-year follow-up of the same case in 1955.) Here there is no need for any psychodynamic hypothesis. The essential difference between this result and the other is that there is no detectable restriction in the patient's life which might account for the disappearance of manifest symptoms. The result can thus be classed as 'indistinguishable from true resolution'.

The caution which is expressed in the words 'detectable' and 'indistinguishable' in the above paragraph is very necessary. This is because neurotic restrictions may not be at all obvious, and in fact may only be detectable with the aid of a quite far-reaching and sophisticated psychodynamic hypothesis. Nevertheless, such restrictions do exist. The following is an interesting example:

The patient is a man in the late thirties. If he had been examined one year ago, the following would have been found: He has been more successful in work than any other member of his large family. He has always wanted to be the best at everything, has never allowed himself to turn out work which is anything but perfect, and has always (though only in his own mind) been extremely critical of work done by others. When he gets home in the

evening he feels compelled to put in several more hours' work in the house, and he feels unable to relax unless he has done so. He cannot tolerate anyone helping him in such work, because he must do it his own way. He has always got on well with men under him, but if they get awkward he finds it difficult to deal with them and has almost always avoided an open conflict. Some 10 years ago he married a very attractive woman, with whom on the whole he has a very good relation. He has always enjoyed a completely satisfactory sexual relation with her. He does not at all like taking her out in company, and they mostly stay at home and are content to do so.

Now from a psychodynamic point of view one can see a theme running through this man's life: intense feelings of rivalry, but an unwillingness to face this rivalry coming out in the open. One might immediately think of an Oedipal problem, or of sibling rivalry, and the obsessional features would suggest a defence against guilt about this. At the same time he has apparently no greater difficulties than the average person, and few people would recommend him for treatment.

Yet the whole significance of these small disturbances is radically altered by the subsequent history of this patient. The work he was on came to an end, and, because there was not enough work to go round, he was assigned as unofficial assistant to a colleague. He was now forced to co-operate, in a somewhat inferior position, with another man; his perfectionism was curbed, and he began to experience intense petty irritations to which he could give no expression. From this time he became impotent.

One way of treating such a patient might be to ask his employers to put him back on a job of his own. He might then return to the same psychodynamic position which he held before his breakdown, become potent again, and be regarded as 'recovered'. Yet as far as the psychodynamic problem is concerned, this kind of result, based on the relief of external stress, cannot be regarded as anything but a 'false solution'. And it must be remembered

that this 'false solution' leaves him in exactly the same state as immediately before his breakdown, so that the possibility of further breakdown still remains.

In fact, when this man had had a few sessions of psychotherapy in which he came to realize something of his problems over rivalry, he said that he now felt that all this was 'silly' and 'wrong', that he had made up his mind to get on better with his colleague, that he had recovered his potency, and need he come any more? This had all the hallmarks of a 'false solution' by 'flight into health', which was confirmed a few weeks later when his impotence returned as badly as ever.

On the other hand, if this man, like the homosexual patient described above, had reported certain other changes in addition to the loss of his presenting symptom, then there would be some justification for talking in terms of partial 'resolution'—for instance if he had now become able to deal firmly with a man under him who was being awkward; or to take his wife out in company without any strain, or to sit around and read all day over the week-end if he felt like it. I do not believe that any of these changes would be manifestations of a flight into health. Still less would I believe it if several of these changes occurred together. Moreover, this would mean that after treatment he was *better than before his breakdown*, which thus becomes one of the important indications of 'true resolution'.

The example of this patient suggests two other possible kinds of change which might be used with comparative assurance as criteria of 'resolution':

- (1) Changes in certain kinds of neurotic behaviour patterns, which are specific for a given patient;
- (2) Changes in several different areas of a patient's life in consequence of a single piece of therapeutic work.

An example quoted by Bandler (1948) emphasizes that such results have to be treated with the greatest caution:

The patient was a woman of 40 suffering from a number of depressive symptoms. He

treated her according to his therapeutic plan of analysing the 'morbid identification to her dead mother'. This was apparently successful, she became symptom-free, and he decided to present her at a case conference. On the day of the conference she suddenly got the idea that he was presenting her to get advice because he did not know what to do with her, and all her symptoms returned. He told her that, on the contrary, he was presenting her in order to help others, because he understood her so well. She immediately became symptom-free once more.

This example leads to another important point. In this particular patient a number of different symptoms disappeared after interpretative therapy. This fact might prejudice anyone dynamically oriented in favour of assessing the result as 'resolution'. Yet the subsequent history of this case shows that the same result can follow on reassurance. This emphasizes that when a result follows on such apparently 'psychodynamically valid' occurrences as acquisition of insight, or abreaction of feeling, this fact cannot be used as evidence in favour of 'resolution'. But the converse applies also: when a change occurs without insight or abreaction, this fact cannot be used as evidence that the change is a 'false solution'. If this were not so, the majority of changes which occur without treatment would have automatically to be regarded as 'false solutions', which would be begging the whole question. The result should ideally be judged only on the changes themselves, *alone*. But in our present state of relative ignorance of how to distinguish between 'false solution' and 'resolution' this is hardly possible. The most that can be said is that further research is needed into which kinds of change stand up to the test of time and which do not.

These examples imply the principle that in order to assess a psychotherapeutic result we need evidence from the patient's life rather than from his behaviour in therapy or his response to projection tests. It would indeed be some indication of improvement if the impotent patient described above had been

able to express his rivalry with the therapist in desiring some imaginary woman; or had been able in a second T.A.T. to tell stories about three-person situations (which he had been quite unable to do in his first T.A.T.); but this would be of little use without evidence that he was better able to cope with rivalry in his outside life. This is not to decry the importance of projection tests; for these may be essential to the formation of an accurate psychodynamic hypothesis, and may, in doubtful cases, give valuable additional evidence about whether a change in the patient's outside life is in fact due to 'resolution' or 'false solution'.

Two possible objections to this approach, however, are as follows: (1) that for criteria taken from the patient's life one has to rely almost entirely on the patient's word; and (2) that such criteria may often be extremely intangible.

The fact that one has to rely on the patient's word is not a serious objection. All methods based on questionnaires answered by the patient, such as the 'self-ideal correlation' of Rogers & Dymond, are open to the same objection. The advantage is that the answers are quantifiable, and they are no more objective than answers given by word of mouth. Both need to be interpreted, in an essentially subjective way, in the light of the overall situation.

The other objection, that the criteria are intangible, can also be overcome. There comes to mind one of Rogers & Dymond's successful cases, 'Mrs Oak', who at the end of therapy said that she had suddenly stopped rejecting the feminine role, as if by magic—'and there's a difference, believe me, in the way a man lights my cigarette'. To use this remark as a significant indication of change might seem to a hard-headed scientist quite laughable, yet it gives information which cannot be conveyed by the fact that her self-ideal correlation increased from 0.21 to 0.79.

One of the fundamental difficulties in this field is that the psychotherapist has to be both an intuitive artist and an objective scientist; and that whereas the artist may have the truth but be unable to demonstrate it, the scientist

may only be able to demonstrate something which has little bearing on the truth. A basic problem in the study of psychotherapy is that of combining the functions of the two.

There is a danger in the fact that it is always very easy, at the end of therapy, to point to some odd remark by the patient and to maintain that it indicates some tremendous improvement. The advocates of objective criteria will write this off with contempt, and rightly so. The way to avoid this kind of arbitrary *a posteriori* judgement, however, is to make it *beforehand*. To use Rogers & Dymond's example: we would be much less inclined to reject Mrs Oak's remark as an indicator of significant change, if the authors had carefully studied the ways in which she rejected femininity, before the start of therapy, and had laid down as one of the criteria of successful therapy that her behaviour must change in a definite direction.

C. *The therapeutic value of 'false solutions'*

In the previous section I have implied that 'resolution' is always preferable to 'false solution'. Theoretically this is so, but the value to the patient of certain kinds of 'false solution' should not be underestimated. A fundamental aim of psychotherapy is to reduce the strain and to increase the satisfaction in the patient's life. Sometimes 'false solutions' may achieve this, and then the distinction from 'resolution' becomes relatively academic. This applies particularly to the important beneficial influence of some 'false solutions' on the future history of the patient. A benign circle may be set up, or a vicious circle prevented. If, for instance, the impotent patient described above had managed to recover his potency indefinitely by reduction of external stress or flight into health, then much harm may have been prevented in his relation with his wife. Similarly, a patient, under the influence of some temporary change in psychodynamics, may be enabled to take some decision or perform some action which might have an equally good effect upon the future. For an example

of this, see the 'unsuccessful accountant' described later.

D. *Summary of basic principles for the psychodynamic assessment of results*

The above considerations may now be summarized in a number of principles:

(1) Any true assessment depends on a knowledge of all the disturbances in a given patient and the change in each of them.

(2) Such disturbances may be manifold, subtle, and unknown to the patient himself.

(3) These subtle disturbances may therefore only be found with the help of an hypothesis about the psychodynamics of the case.

(4) The most important evidence comes from the patient's behaviour in his outside life, and evidence from tests or his behaviour in therapy should only be used to supplement this.

(5) The point of actual breakdown cannot usually be regarded as the beginning of the patient's illness, and therefore the true 'baseline' from which to judge improvement is the patient's position before breakdown.

(6) On the basis of our as yet limited knowledge, criteria should be laid down which must be fulfilled if the changes in the patient are to be tentatively regarded as indicating 'true resolution'.

(7) These criteria should be described as far as possible before treatment is started.

(8) The result is judged qualitatively in psychodynamic terms on the basis of these criteria.

(9) Although occurrences in therapy cannot be ignored, and may often be of great help in forming a psychodynamic hypothesis about the nature of the result, these should as far as possible not be allowed to over-ride the criteria laid down before treatment.

E. *'Objective' methods of assessment in the light of the above considerations*

The arguments given in the previous sections have, I hope, served to emphasize the large quantitative and complex qualitative differences between results. The question is how

far the 'objective' methods of assessment can do justice to these differences. As a representative example I shall use the 'self-ideal correlation', which seems to me to be the best of these methods. It has the quantitative advantage that it can be made to cover a number of different areas, and ranges continuously between -1.00 and $+1.00$.

If it is true that 'resolution' is often accompanied by changes in several different areas of a patient's life, whereas 'false solution' is more often accompanied by changes in only a single area, then the two kinds of change may at any rate be in part distinguished quantitatively in the overall result. On the other hand, it is my contention that the questions asked in a general test of this kind can rarely be specific enough for a true psychodynamic assessment of a given patient. As an example, a very important criterion of 'resolution' in Rothenberg's dentist patient, described above, would be if he lost not only his hiccups but also his fear of anaesthesia. One cannot imagine a general test asking this; nor, if it did, giving enough weight to this key question for this particular patient. This is a basic objection to this kind of method.

It is possible that this difficulty could be overcome by devising a test specifically based on a psychodynamic hypothesis about each particular patient. An essential preliminary to such a method would be the development of a qualitative method based on the principles described above. This is described and illustrated in the next section.

THE ASSESSMENT FORM

The principles described above have been embodied in a form which summarizes the minimum knowledge required in the assessment of changes in a given patient. This is a development of forms devised by Dr Balint and extensively used by his 'Workshop on Brief Psychotherapy' at the Tavistock Clinic. Five examples of the use of this form on patients seen by members of this Workshop, and in whom some kind of 'improvement' occurred, are given below.

Considerable resolution

A. Details of patient

Pseudonym: the neurasthenic's husband. A married man in the 50's complaining of impotence, sexual preoccupations, and difficulties in relation to people for 17 years.

B. All known disturbances in patient's life

All come under the heading of inability to assert himself.

(a) *Relation with wife.* Wife is a very disturbed person and he is quite unable to deal with her. He behaves as a slave to her needs and does not think of his own needs at all, but feels constant resentment at this. This applies particularly to the sexual relation—he has intercourse only when he feels she wants it, and when he does he cannot satisfy her. He seeks his own satisfaction in masturbation. He submits to her in the home; does the women's chores: does what she tells him. He does not know whether he can stand her any longer or whether he can do without her. He feels like a small boy: unhappy and despairing.

(b) *Relations at work.* Although he is good at his work he is extremely self-depreciatory in his relations with colleagues and superiors. He allows himself to be paid at far less than his true value. He feels insecure, regards himself as lucky to have the job, and is always afraid he may lose it.

(c) *Relations with children.* He feels his children have no respect for him. In fact they always consult his wife rather than him.

C. Minimum psychodynamic hypothesis required to explain B

Since in this case all known disturbances come under the single heading of inability to assert himself, there is no need for a psychodynamic hypothesis in the assessment of the result. The hypothesis was made, however, that he had denigrated his father and then felt unable to be more of a man than he. Confirmatory evidence for this was obtained during therapy.

D. *Evidence required in assessment of result*(1) *'Ideal' result*

Evidence that he is able to assert himself, spreading throughout his whole life, e.g.

(a) *Relation with wife.* Increased potency: ability to serve in intercourse not only his wife's needs but his own; ability to assert himself in the home, e.g. no longer does the women's chores, sometimes makes his wife do what he wants, etc. Ability to be angry appropriately.

(b) *Relations at work.* Ability to value himself more highly, to be self-seeking and self-assertive: to enjoy taking charge of things.

(2) *Discussion of value of partial results*

Any move towards self-assertiveness will be of value.

E. *Changes in all disturbances listed under B*
(14 Sessions. Follow-up 18 months.)

(a) *Relation with wife.* Although the difficulty in dealing with his wife remains, he has been able to assert himself in many ways. He feels no need to submit to her and is often able to handle her in such a way that she does not even try to dominate him. He is able to make her do things his way. He no longer does the women's chores. Sexually, he is still unable to satisfy her, but he is now able to assert himself better and to attend to his own needs and not only hers. Masturbation has ceased. He has decided to stay with his wife. He no longer feels in despair, but can accept the limitations of his situation.

(b) *Relations at work.* He remains critical of his work situation but is now not unhappy and can look forward to the future with more reasoned ideas. He has had two large increases in salary, both of which he got by demanding to be paid for what he was worth. He has been offered, and has accepted, a more responsible position. He is no longer in fear of losing his job.

(c) *Relations with children.* He now feels they respect him. In fact his son has recently remarked that the patient has had to bear a

great deal of responsibility and has borne it well.

F. *Tentative psychodynamic assessment of result*

He was suffering from a kind of neurotic behaviour pattern that one would not expect to change substantially except under the influence of 'resolution' of a neurotic conflict. The possibility remains that the change might be largely on the basis of an identification with the therapist's values, but this is rendered unlikely by the fact that (1) there has been no relapse on termination, and (2) the changes have been maintained over a long period without any further relation with the therapist.

Moderate resolution

A. *Details of patient*

Pseudonym: the lorry driver. A married man in the early 30's complaining of severe anxiety attacks after a minor accident to his lorry.

B. *All known disturbances in patient's life*(1) *Background problems*

(a) *Compulsive phenomena* (gradually increasing over last few years). It is not easy to give an impression of the extent to which this man's life was affected by compulsiveness, because all the things that he felt compelled to do were in themselves perfectly 'normal'. The difference from 'normality' lay in the degree of tension which he suffered if he failed to give in to his compulsions.

At work, often feels compelled to write down even the simplest instructions to make sure he's got it all right. Unable to refuse overtime. As a result, grossly overworks, earning £20-25 a week. If his departure is delayed he has to find some other job to do. At home has to be constantly occupied. Feels compelled to finish any job at once—e.g. if the toaster goes wrong he has to mend it before breakfast. Feels compelled to go to bed early and get up early. If his wife is preparing a meal feels compelled to help her.

(b) *Relation with mother.* Considerably affected by compulsiveness. Feels compelled to visit her as often as his wife can be persuaded to go—2–3 times a week.

(2) *Breakdown into acute anxiety*

(c) *Severe anxiety and confusional attacks.* Has had about three. Feels his head is in a jumble; that he will pass out; once wrote his name and address on a bit of paper in case he was picked up unconscious.

(d) *Less severe anxiety and confusional states.* These, which may last for days, are set off by situations of anger, delay, conflict, noise, e.g. in traffic jams, when a wireless blares, at football matches. He has had a row with a neighbour and feels tense whenever he gets home.

(e) *Relations with his children.* If they make a noise, are naughty, or make demands on him, he loses his temper. He has thrashed his son on several occasions.

(3) *The following developed during the therapy period*

(f) *Phobias.* Complicated mixture of agoraphobia and claustrophobia. Fear of the open road—can drive his lorry in London but not on long-distance journeys. His employer has adjusted his work accordingly. Yet also has a fear of enclosed spaces, and likes to be driven by his wife into the country in his car. Does not like travelling alone or in trains.

(g) *Depressive phenomena.* Crying attacks, suicidal thoughts, fear that he may commit suicide.

C. *Minimum psychodynamic hypothesis required to explain B*

There is evidence that he has a severe problem over guilt about aggression. Therapy suggested that this concerned anger with his mother.

D. *Evidence required in assessment of result*

(1) *'Ideal' result*

Loss of all the above symptoms. Ability to control his aggressiveness but to be aggressive in an effective way when the situation demands

it. There should be no restrictions such as those imposed by his phobias.

(2) *Discussion of value of partial results*

Too complex to discuss.

E. *Changes in all disturbances listed under B*

(16 sessions. Follow-up 18 months.)

(a) *Compulsive phenomena.* Now does not do much overtime and is perfectly content to earn £14 a week. Does not feel compelled to do jobs around the house at once—'My wife says I'm getting quite lazy.' Less compelled to check things at work. Still not happy unless he can occupy himself, but has replaced tensely compulsive activities by activities which may still be somewhat compulsive but are more in the nature of 'reparation', e.g. has taken to mending shoes as a hobby, and has taken an orphan into his home.

(b) *Relation with mother.* Has had it out with her, and their relation has considerably improved. She has discussed his childhood with him. He now no longer feels so compelled to visit her, and goes once a week or once a fortnight.

(c) and (d) *Acute and chronic anxiety and confusion.* Has had no severe anxiety attacks. He still gets somewhat tense in some of the same situations as before, but instead of lasting for days this rarely lasts more than a quarter of an hour. He is able to control it by doing something else, and on at least two occasions to disperse it by analysing his own feelings.

(e) *Relation with children.* Because he works less overtime now he sees them almost every night, and the relation with them has enormously improved. He reads to them and lets them climb all over him. He copes realistically with their demands for more—'just one more story and then up to bed'. When they are naughty he deals with them firmly, and sends his son off to bed instead of thrashing him.

(f) *Phobias.* These remain largely unchanged. He still gets anxious when closed in, and would not like to have to travel by train. He refuses

to go back to long-distance driving, saying he never liked the job, and it looks as if he could not go back if he tried.

(g) *Depressive phenomena.* These have not recurred.

F. *Tentative psychodynamic assessment of result*

There seems strong evidence that

(1) A good deal of his obsessional neurosis has been resolved, though some still remains. Compulsive phenomena have to a large extent disappeared or been replaced by reparative activities from which he can get some satisfaction.

(2) Manifest anxiety has greatly decreased, but a residue remains in his phobias. The meaning of these is not clear.

Taking his state before breakdown as a baseline, therefore, we can say that his obsessional personality has been considerably resolved, but that a new symptom has appeared in his phobias. These phobias are undoubtedly much easier to bear, but it is not clear how much psychopathology is locked up in them, so that no absolutely certain assessment can be made.

Limited resolution

A. *Details of patient and therapist*

Pseudonym: the homosexual schoolmaster. An unmarried man in the early 20's, complaining of a sudden attack of uncontrollable homosexual feelings.

B. *All known disturbances in patient's life, with evidence*

(1) *Known at initial assessment*

(a) *Homosexual breakdown.* Soon after a girl, teaching at the same school, had asked him to a dance, he felt impelled to go to a coffee bar where he knew homosexual contacts were made. He met another young man. They were caught touching each other's genitals in public. No previous homosexual feelings.

(b) *Inhibited heterosexuality.* Largely unaware of heterosexual feelings. Masturbates without phantasy: nocturnal emissions with-

out dreams. Never tried to have a girl friend, though mixed freely with both sexes at school and training college.

(c) *Relations with older men.* Unable to stand up to them; uneasy with them; constantly worried about making small mistakes at work. Afraid that if he takes the girl out the headmaster will find out about it.

C. *Minimum psychodynamic hypothesis required to explain B*

Fear of being punished for heterosexuality by a male authority, with flight into homosexuality.

D. *Evidence required in assessment of result*

(1) *'Ideal' result*

(a) His life no longer seriously affected by homosexual feelings.

(b) Active attempts at forming relations with girls, with ultimate success.

(c) Ability to stand up to older men; no uneasiness in their presence; no further worry about small mistakes.

(2) *Discussion of value of partial results*

(a) If a reduction in homosexual feelings is to be regarded as resolution it must be accompanied by a corresponding increase in heterosexual feelings.

(b) Marked improvement in his relation with older men, without changes in the sexual problem, is very difficult to assess, and may well be a 'false solution' based on placation of the male authority by retreat from heterosexuality. Yet if the relation with male authority does not have the quality of placation, this result would be more likely to be due to 'limited resolution'.

E. *Changes in all disturbances listed under B (26 sessions. Follow-up 8 months.)*

(a) *Homosexual feelings.* Now has no wish to seek a homosexual relation actively, but has homosexual feelings at times, and 'I don't know what I'd do if someone tried to seduce me'.

(b) *Heterosexual feelings.* Made two half-hearted attempts at forming relations with

girls, neither of which lasted, but without any increase in homosexual feelings or fear of male authority. No other change.

(c) *Relations with older men and work.* Has considerably improved in his ability to stand up to older men—in fact has gone rather the other way, and has treated his headmaster with aggressiveness and a certain lack of consideration. Great increase in his confidence and ability to enjoy his work. No worry over small mistakes.

F. *Tentative psychodynamic assessment of result*

According to above criteria, probably resolution of a limited aspect of his problem, namely his relation with older men; without any evidence for change in the more important problem, the flight from heterosexuality.

Valuable false solution

A. *Details of patient and therapist*

Pseudonym: the unsuccessful accountant. A married man in the early 30's complaining of an inability to get a job.

B. *All known disturbances in patient's life, with evidence*

(a) *Difficulties over work.* Has been unsuccessful in his present work as junior partner in a firm of accountants. Wants some kind of personnel work, but has been turned down for about twenty jobs during the last 2 years. Feels very tense at interviews.

(b) *Character problems.* Very difficult to get any survey of his difficulties apart from those above. The interview suggests that he has great difficulty in making real contact with people of both sexes.

C. *Minimum psychodynamic hypothesis required to explain B*

The interview and T.A.T. suggests that a major problem is a conflict between extreme competitiveness and an extreme fear of competition. The T.A.T. suggests severe problems over aggressiveness in general.

The hypothesis was made that some difficulty arising out of the above problems appears at interviews and puts people off accepting him for jobs.

D. *Evidence required in assessment of result* (1) *'Ideal' result*

(a) He should get a job which satisfies him and should make progress in it, in accordance with his undoubted abilities.

(b) There should be extensive evidence from his life of solution to problems over competitiveness, aggressiveness, and human contact.

(2) *Discussion of relative value of partial results*

Assuming that his emotional problem at interviews is the chief factor preventing him from getting jobs, evidence required for partial resolution of this problem would be (a) that he gets a job and holds it; (b) that this was due to altered behaviour at interview; and (c) that such altered behaviour always shows itself in similar situations in future.

E. *Changes in all disturbances listed under B* (7 sessions. Follow-up 2 years.)

(a) *Difficulties over work.* Obtained a job during therapy, not personnel work nor to do with human relations; at a much lower salary than that he would have attained had he been successful in his profession; in which, since he is alone in this position in the firm, there is no fear of competition; but which makes use of his training. Has held the job and made adequate progress in it. Seems perfectly content and gets pleasure from his work.

No evidence that this was due to altered behaviour or feelings at interview. More likely due to the fact that this was a more realistic choice of job than before.

(b) *Character problems.* No evidence of change in any of these.

F. *Tentative psychodynamic assessment of result*

The psychodynamic change seems to be that he has now made a retreat from com-

petitiveness. In other words he has undergone a psychodynamic reorganization with no evidence for any resolution; but which may be a good and realistic solution in view of his severe basic problems.

Valueless false solution

A. *Details of patient and therapist*

Pseudonym: 'Tom'. A boy of 16. Complaint: Acute anxiety with physical symptoms

B. *All known disturbances in patient's life, with evidence*

(a) *Attacks of physical symptoms (4 weeks).* Cannot see properly; feels he is falling; heaviness in chest; breathing difficulty; pain in left half of face; feels his arms have gone dead.

(b) *Anxiety (4 weeks).* Feels there is something serious wrong with him physically; that he is going blind; that he will die, etc.

(c) *Character problems, etc.* Insufficient evidence.

C. *Minimum psychodynamic hypothesis required to explain B*

Mother has left his father and is now pressing Tom to come and live with her. Severe conflict over this.

D. *Evidence required in assessment of result* (1) *'Ideal' result*

(a) Ability to make up his mind finally whether to live with mother or father: complete loss of symptoms and anxiety; no apparent substitute disability; no further breakdown. Ability to maintain relations with both parents.

(b) Regular work, with advancement according to his potentialities.

(c) Ability to form satisfactory relations with people of both sexes, and above all to tolerate 3-person situations, in the future.

(2) *Discussion of value of partial results*

If his symptoms and anxiety clear up, the crucial test will be whether or not he is still able

to tolerate having a relationship with *both* parents. If this is not so the result will be regarded as a false solution.

E. *Changes in all disturbances listed under B* (Four sessions; patient broke off treatment; follow-up 3 months.)

Patient left his father and went to live with his mother. All his symptoms and anxiety cleared, only to return with equal force 1 month after termination. Still refuses to go anywhere near his father, or to return to original therapist. Treated at another hospital.

F. *Tentative psychodynamic assessment of result*

A clear-cut case of false solution by relief of external stress.

POSSIBLE FUTURE RESEARCH

The arguments put forward in this paper lead to one conclusion whose importance overrides all others; that the biggest gap in our knowledge is concerned with the psychodynamics of change in untreated patients. Until this gap is filled there seems to be no possibility of assessing the true value of psychotherapy.

A second conclusion is that studies of treated and untreated patients using crude rating scales are of little further value; and that a far greater contribution can be made by qualitative studies of even only a few patients with a method of assessment based on psychodynamics.

Moreover, the difficulty of providing a series of untreated patients for such a study has been exaggerated. This has arisen from the supposed need for the treated and untreated patients to be exactly comparable, which immediately raises almost insoluble ethical problems. But all work which has been done on this problem indicates that untreated patients, in whatever way they are selected, show a marked tendency towards improvement. Therefore the results with *any* series of untreated patients should be of great interest. In Saslow & Peters's work,

for instance, the untreated patients were those that selected themselves during the course of routine Clinic work. A psychodynamic study of the changes in such a series, and a comparison with a treated series, is not only feasible but would be one of the most important contributions which could be made to this whole field.

Some of the additional investigations which such a study might allow are as follows:

(1) A comparison between the changes due simply to maturation or change of environment with those that occur as a result of treatment. This might lead to criteria by means of which the two kinds of change can be distinguished.

(2) A comparison between the kinds of change that are lasting and those which are shown by subsequent events to be unstable. This might lead to criteria for distinguishing between 'false solution' and 'resolution' before the end of a long follow-up.

(3) A comparison of the time-scale of changes with and without treatment. It may well be that one of the functions of psychotherapy is to hasten changes which would occur, but only much more slowly, without treatment.

Finally, if I may be allowed to speculate for a moment, I suspect that when improvements in treated and untreated patients are compared, it may be found that while many show no differences, a proportion of treated patients improves to an extent quite unobtainable without treatment. These patients comprise the 'specific' results of psychotherapy, and give the therapist his justification and much of his reward for his hours of unspectacular work with the remainder.

SUMMARY

1. A review has been made (a) of previous methods of assessing psychotherapeutic results, and (b) of previous estimates of the improvement rate in neurotic patients, which show practically no difference whether the patients were treated or not.

2. It is suggested that as in a physical illness

any assessment of a therapeutic result should be based on a knowledge of pathological physiology or anatomy, so in a neurotic illness such an assessment should be based on a knowledge of pathological psychodynamics. Since most previous methods are not so based, results obtained with them tend to be inaccurate. This may be the reason for the lack of the difference observed between treated and untreated patients.

3. An attempt is made to overcome the difficulty of the apparently arbitrary nature of psychodynamic theory by (a) setting down all the material about a given patient, and (b) forming a psychodynamic hypothesis for each individual patient which is the very minimum needed to account for the evidence, and the minimum needed for an accurate assessment of the therapeutic result.

4. Criteria can be laid down before therapy which must be fulfilled if the result is to be regarded as a real psychodynamic improvement. Examples of the use of this method are given.

5. It is suggested that this leads to a partial solution of the problem of providing 'control' patients, and hence to a relatively practicable way of obtaining much more accurate evidence about the value of psychotherapy.

ACKNOWLEDGEMENTS

The considerations put forward here have all arisen out of work being done by many people at the Tavistock Clinic, London, and the Cassel Hospital, Richmond. I am indebted especially to Dr M. Balint, Consultant Psychiatrist at the Tavistock Clinic, and all members of his Workshop, who have discussed these problems many times in relation to the results of Brief Individual Psychotherapy; and to Dr J. D. Sutherland, Medical Director of the Tavistock Clinic, and his Seminar, who have discussed the problem in relation to the results of Group Therapy. The method of assessment used by Mr Herbert Phillipson, Principal Psychologist of the Tavistock Clinic, in his paper 'The assessment of progress after at least two years of Group Therapy' (1958) embodies, quite

independently, several of the ideas discussed here.

I would also like to say that there is one projected study which by implication takes many of the above arguments into account. This is the recent report by the Menninger Foundation on their Psychotherapy Research Project (Wallerstein, Robbins, Sargent & Luborsky, 1956). My reading of this report formed the starting-point for many of the ideas

expressed here. This applies particularly to the idea of an inventory of the patient's disturbances under psychodynamic headings, together with predictive studies, as essentials to a true assessment of results.

I am indebted to Dr T. F. Main, Medical Director of the Cassel Hospital, and Mr J. L. Boreham, Senior Clinical Psychologist at the Tavistock Clinic, for permission to use some of their case material.

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PSYCHOLOGICAL FACTORS IN MUSCULAR PAIN

By LEON J. DRASPA*

The present study investigates the theory that some muscular pains, such as 'fibrositis', and 'muscular rheumatism', are of psychogenic origin. One of the oldest theories on the aetiology and morbid anatomy of muscular pain is that of Stockman, who attributed muscular pains to so-called 'fibrositic nodules', said to result from inflammation. This theory has not, however, been substantiated; indeed some authorities conclude, on the basis of biopsies, that fibrositis is non-existent (Ogilvie & Thomson, 1950).

Three main theories are now current: that of increased tension in fatty lobules (Copeman & Ackerman, 1944); that of localized muscle spasm (Elliot, 1944); and that of psychogenic rheumatism (Halliday, 1937, 1941). Halliday was perhaps the first in this country to point to the relationship between psychological factors and muscular pain. According to him, emotional stress and anxiety states may produce pain, stiffness and limitation of movement. These symptoms are considered to be overt manifestations of inner emotional tension provoked by disturbing external events and conflicts. Halliday also suggested that muscle pain and stiffness may be 'symbols' which stand for a mental experience and are, in a sense, a form of body language (e.g. spitting when rejecting, or holding stiff when disapproving).

Although the psychological theory of muscular pain has been challenged by those who defend the organic theories, its validity has been strongly supported by many authors who consider the fibrositic syndrome to be precipitated by a psychosomatic disturbance. This view is supported by observations of the fluctuation of symptoms with changing life

situations, and of their frequent disappearance with the removal of environmental stress. Such a mechanism has long been accepted as a basis for cardiac and gastric complaints, but not for rheumatism (Graham, 1954).

THE PROBLEM

On the basis of empirical observation the following hypothesis was formed: that some muscular pains, described clinically as fibrositis, muscular rheumatism, myalgia, etc., are evoked by muscular tension brought about by covert behaviour. This hypothesis implies, first, that muscular pain is evoked by muscular tension (contraction) (Draspa, 1955), and secondly, that habitual muscular tension is the result of prolonged covert behaviour. The latter contention is the subject of the present paper.

By covert behaviour is meant behaviour which is subliminal from the observer's point of view; even though not seen, it is present, being simply reduced in magnitude to small muscular contractions of which even the patient himself may be unaware.

In order to collect evidence for or against this hypothesis an investigation was made into motives and drives as stimuli eliciting muscular contraction with particular consideration given to:

(a) The personality make-up and especially the attitudes, feelings and pertinent behaviour patterns of the individual.

(b) Those recent life situations which had a distressing effect on the individual and which appeared to serve as the main stimulus for action.

(c) The degree of disruption of behaviour of the individual as the result of the operation of environmental stress.

(d) The individual's attempt to cope with those environmental stress situations which

* This paper is based on part of the late Dr Draspa's thesis for the degree of Ph.D. in the University of Edinburgh.

had been forced upon him (adjustive behaviour).

(e) The individual's bodily preoccupation.

PROCEDURE

The psychological inquiry consisted of two independent investigations. One of these used a standardized interview, the other psychological tests. The results were afterwards compared, and where there was a notable discrepancy between the findings for a given individual, further investigation by means of interview was carried out.

The interview technique and the psychological tests may be summarized and contrasted in the following way:

Interview (standardized) versus psychological tests

(1) *Personality assessment.* On the basis of a prepared scheme of behaviour tendencies as listed by Murray (1938), the outstanding feelings, attitudes and general motor and verbal behaviour patterns displayed by the patient were assessed.

versus

Self assessment. The patient was asked to identify his feelings, attitudes and actions on a questionnaire which contained three of Murray's behaviour variables. These behaviour variables, presented in descriptive form, were chosen on the basis of the assessment by interview and were assumed to represent the most probable pertinent behaviour tendencies of the patient tested. If no variable seemed appropriate to the patient, he was asked to describe briefly his feelings and actions at the time he became ill, or just previously.

(2) *Assessment of patient's disruption of behaviour.* Based primarily on autonomic activities displayed by the patient.

versus

The Minnesota Multiphasic Personality Inventory (M.M.P.I.) (Hathaway & McKinley, 1951).

(3) *Assessment of patient's bodily preoccupation.* A prepared list of points dealing with bodily preoccupation was used.

versus

M.M.P.I.—Hypochondriasis scale.

(4) *Assessment of patient's recent environmental stress.* A history was taken of what happened to the patient just before or about the time of onset of pain.

versus

The Word Association Test (Cattell, 1948).

The standardized interview served as the main technique; for each history in the present study there was a systematic coverage of a basic minimum of about fifty items, but the interviewer did not hesitate to attempt to secure additional data on special situations if such were presented by the patient.

THE MEASUREMENT TECHNIQUES

Measurement of personality factors

The investigation was planned on the basis of action. It is the generally accepted assumption in personality theory that each function or form of behaviour serves an organismic purpose. In other words, behaviour is goal-directed (Lewin, 1935; Klein & Krech, 1951), the goal being some specific relation of the organism to its internal and/or external environment. Since few of these goals are actually realized, they have to be discovered and defined indirectly by pertinent behavioural manifestations which represent an organismic 'need' or 'drive' to achieve a particular goal. The work of H. A. Murray and his collaborators, which rests on these principles, was used as the basis for a behaviour tendency rating scale.

The assessment of behavioural tendencies of an individual was carried out by:

(1) *Rating on a 3-point scale.*

0 = absence of behaviour tendency;

1 = presence in slight degree;

2 = presence in marked degree;

with intermediate stages 1- and 1+.

(2) *Ranking.* Three of the most outstanding behaviour tendencies of a patient were ranked in order 1 to 3.

(3) *Patient's self-rating.* This procedure has been described previously, under the sub-heading of Self Assessment. Ranking was in order 1 to 3.

From these three assessments the pertinent behavioural pattern of the individual was finally established.

Measurement of the patient's disruption of behaviour

A list of signs and symptoms of autonomic disturbances and of general 'emotional' behavioural symptoms was drawn up and the severity of the patient's disruption of behaviour assessed by taking into account the number of symptoms, their severity and repetitions and their interference with normal life.

A three-point rating scale was adopted: 0 = no disruption of behaviour; 1 = some disruption of behaviour and 2 = marked disruption of behaviour. Intermediate stages were rated 1- and 1+.

Findings obtained in this way were supplemented by the Hy (hysteria) scale of the M.M.P.I. The obtained scores on the Hy scale were translated into a five-point scale and compared with similar data obtained from rating. The transformation from *T*-score on the Hy scale to the five-point rating scale was made as follows:

<i>T</i> = 46-54	Rating = 0
55-59	= 1-
60-64	= 1
65-69	= 1+
70+	= 2

Measurement of patient's bodily preoccupation

A list of questions indicating bodily preoccupation was drawn up and the patients rated according to the number and severity of symptoms displayed.

A three-point rating scale was employed: 0 = no bodily preoccupation, 1 = some bodily preoccupation and 2 = marked in-

terest in body functions. Intermediate stages were 1- and 1+.

This rating was supplemented by the M.M.P.I. Hs. scale which represents a measurement of the amount of abnormal concern with bodily functions. The *T* scores of the Hs. scale were translated into a five-point scale, as for Hy, and these values were compared with the data obtained from rating.

Measurement of patient's recent environmental stress

Since the meaning of environmental stress is so very subjective it was only possible to determine, by interview, whether there was any environmental stress and the patient's reaction to that stress. To obtain an objective check the Word Association Technique was used. If the results of this test appeared to confirm the environmental stress defined during the interview, it was taken to be established. If, however, there was a discrepancy between the two techniques, further inquiry was made using as a basis a selection of significant words (about eight to twelve) from the Word Association Test.

THE PATIENTS

The distribution of patients by age and sex was as shown in Table 1.

Table 1. *Distribution by age and sex of patients in the sample, N = 112*

Age groups	Number of patients		
	Male	Female	Total
16-20	0	3	3
21-25	3	3	6
26-30	4	3	7
31-35	5	2	7
36-40	10	3	13
41-45	7	13	20
46-50	6	15	21
51-55	3	9	12
56-60	3	7	10
61-65	4	2	6
66-70	4	1	5
71-75	1	1	2

RESULTS

Onset of pain and factors involved

Patients reported muscular pain to have come on under three conditions: after illness (5 cases); after accidents (16 cases); and 'just came on' without any particular reason (91 cases). Among factors which seemed to facilitate the onset were physical and psychological factors. More than one factor was often involved, but usually one of these appeared to be the precipitating factor. A distinction was therefore made between primary and secondary factors.

Table 2 shows the number of patients and the factors involved in the onset of their pain.

Table 2. *Factors involved in onset of pain*

	As primary factors	As secondary factors
Physical factors		
Organic	5	8
Mechanical	23	3
Fatigue	0	41
Psychological factors		
Adaptive behavioural pattern in relation to external environment	83	7
Adaptive behavioural pattern in relation to internal environment	0	28
Bodily preoccupation	0	38
Bodily preoccupation combined with malin- gering	0	24
Unknown factors	1	—

Although only 38 cases in the sample displayed habitual excessive bodily preoccupation, all patients showed concern about their well-being to some degree. This degree of bodily concern was closely related to anxiety, i.e. anticipated danger (e.g. 'Will I ever get better?'). In no case could bodily preoccupation be regarded as a primary aetiological factor. The association between degree of pain and bodily preoccupation is shown in Table 3. (This has been reduced to a 3×3 contingency

table with Garrett's correction, and yields a contingency coefficient of $G = 0.606$, standard error = 0.094.)

Table 3. *Association between degree of pain and bodily preoccupation, N = 112*

		Degree of bodily preoccupation				Total
		1-	1	1+	2	
Degree of pain	1-	—	—	—	—	—
	1	2	5	—	—	7
	1+	2	4	18	4	28
	2	—	8	11	58	77
Total		4	17	29	62	112

Disruption and adjustment of behaviour

Adjustive behaviour was only considered to take place when some disruption of behaviour has occurred.

The disruption of behaviour is shown in Tables 4 and 5.

Table 4 shows the relation of disruption of behaviour to degree of pain. The contingency coefficient, calculated as before, is $C = 0.776$, with a standard error of 0.094.

Table 4. *Association between degree of pain and disruption of behaviour, N = 112*

		Degree of disruption of behaviour				Totals
		1-	1	1+	2	
Degree of pain	1-	—	—	—	—	—
	1	2	3	2	—	7
	1+	1	3	20	4	28
	2	—	3	8	66	77
Totals		3	9	30	70	112

Table 5 summarizes the varieties of disruption of behaviour reported by the patient sample.

The patients' adjustment to stress is summarized in Tables 6 and 7 which also show:

(a) The form of environmental stress.

Table 5. *Outstanding autonomic disturbances and affective actions displayed by patients with muscular pain, N = 111**

Disturbances	In patients of psycho- logical onset, N = 83		In patients of physical onset, N = 28	
Sleep disturbances	76	92	22	79
Headaches and weariness	75	90	16	57
Restlessness	66	80	17	61
Fatigue	62	75	10	36
Excessive worry	54	65	12	43
Constipation	51	61	11	39
Muscle twitching	49	60	9	32
Tingling, burning and 'going to sleep' sensation	42	51	11	39
Boredom and depression	38	46	4	14
Precordial pain	35	42	5	18
Stomach trouble	34	41	11	39

* One patient could not be classified into either category (see Table 2).

(b) The outstanding behavioural tendencies in the patient's personality.

(c) The pain-affected muscles and their main actions.

Assessment of cure as a test of the hypothesis

It has been assumed in the original hypothesis that muscular pain is caused by muscular contraction as an implicit sustained response, under stress, to a noxious environment (internal or external). Thus, since some muscular pains were assumed to be due to psychological facilitation, the experimental group, in addition to their normal physical treatment (directed mostly to the result), received further treatment (desensitization) directed to the *cause* with the intention:

(a) Of disrupting such acquired motor behavioural patterns by first passive, then active relaxation (Draspa, 1955).

(b) Of reassuring the patient that the pain is only muscular and that there was no immediate danger (supportive therapy).

(c) Of giving insight into the causes of excessive muscular contraction and so pro-

moting self-adjustment to changed internal or external environmental situations (self-directive therapy).

The matched control group received physical treatment only.

In both the experimental and the control group the assessment of the degree of recovery was made by the same consultant physicians and surgeons who, in the first place, diagnosed the patients as suffering from muscular pain.

The obtained results on assessment of cure were as follows:

(1) Data relating to *degree of recovery* are presented in Table 8.

The eight patients in the experimental group in whom there was no improvement were found to be confronted with very serious stress situations (e.g. divorce, illness of husband) still operating and there was no possibility of self-adjustment to these new environmental conditions, unless the environment itself could have been 'manipulated' for them.

(2) Table 9 shows *time in which recovery took place*.

Table 6. *Adjustive behavioural tendency in psychological facilitation of pain.*

Muscles involved	No. of cases	Main action of involved muscles	Form of environmental stress	Patient's behavioural tendency towards the stress situation	Accompanying covert behavioural tendency	No. of cases with high scores on Murray variables as noted
Rectus capitis posterior major et minor	7	Extend the head (head retraction) and turn the face towards the same side	1. A task 2. A rival	Pain in neck 1. To master it rapidly and as well as possible 2. To rival and surpass others. To be the first, to be 'on top', to be above everyone and everything To make up for failure by re-striving	To keep the head high	Achievement 7 Autonomy 5 Dominance 4 Aggression 3 Defendance 2
Levator scapulae	3	The cervical part of the vertebral column being fixed, it elevates the scapula	Failure		To 'pull' oneself out of difficulty, to elevate oneself by shrugging the shoulders	Counteraction 3 Defendance 2 Achievement 2 Infavindance 1
Supraspinatus and deltoid	21	Abduction of shoulder and also flexion and extension of arm	Pain in shoulder 1. Aggression 2. Assault 3. Belittlement 4. Censure 5. Criticism 6. Blame	To defend oneself against depreciating and belittling judgements	To assume a defending and forceful posture	Defendance 20 Achievement 13 Autonomy 9 Dominance 9 Succorance 3 Harmavoidance 3 Counteraction 2 Retention 2
Extensors of the wrist: extensor carpi radialis longus et brevis; extensor carpi ulnaris	5	Fixation and extension of wrist	Pain in arm Threat of deprivation	To refuse to give, to withhold	To grip tightly	Retention 4 Acquisition 2 Achievement 2 Autonomy 2 Rejection 1 Acquisition 2 Achievement 2 Aggression 1
Flexors of the wrist: flexor carpi ulnaris, flexor carpi radialis	2	Flexion of wrist	Obstacles preventing gain of possessions and property	To grasp, to snatch, aggressively acquiring	To grasp	

Table 6 (cont.)

Muscles involved	No. of cases	Main action of involved muscles	Form of environmental stress		Accompanying covert behavioural tendency	No. of cases with high scores on Murray variables as noted
			Pain in chest wall	Patient's behavioural tendency towards the stress situation		
Serratus anterior	5	Abduction and upward rotation of scapula	Repellent person, To separate oneself from an object or situation	To push away	Rejection	5
Diaphragm and sternocostalis	5	Muscles of 'inspiration' and also support expulsion, e.g. sneezing, laughing, crying	1. Loss of some-one dear 2. Being rejected	To crave for affection, tenderness and recognition	Harmavoidance Infavoidance Aggression Succorance	3 3 2 2
Quadratus lumborum. Tensor fasciae latae	18	Elevation of pelvis (reversed action) and trunk flexion. Abduction of thigh with internal rotation (from flexed position).	Pain in back Rejection Failure Belittlement Ridicule	To avoid situations which may lead to a lowering of self-regard	Succorance Harmavoidance Abasement Infavoidance Blamavoidance Rejection	5 4 2 2 1 1
Psoas and Iliacus	6	Hip flexion	Opposition Annoyances Attacks Insults	To overcome opposition forcefully	Infavoidance Aggression Dominance Defendance Achievement Autonomy Infavoidance Harmavoidance	6 6 4 3 3 2 2 1
Vastus intermedius	1	Knee extension	Pain in knee As under Psoas and Iliacus	To kick	Aggression Dominance Defendance Achievement Autonomy Infavoidance Harmavoidance	6 4 3 3 2 2 1
Biceps femoris, semitendinosus, semimembranosus	10	Knee flexion	Pain in gluteal region Censure. Retribution from Society	To avoid blame or rejection	Blamavoidance Succorance Inferiority Defendance Nurturance Harmavoidance Autonomy	9 6 3 4 3 2 1

Table 7. *Adjustive behavioural tendency in physical facilitation of pain*

Muscles involved	No. of cases	Main action of involved muscles	Form of environmental stress	Patient's behavioural tendency towards the stress situation	Accompanying covert behavioural tendency	No. of cases with high scores on Murray variables as noted
When organic factors were operating						
Scattered	5	Various	Pain and discomfort	To improve. To get rid of the trouble	To protect. To avoid pain	Harmavoidance 5 Infavoidance 3 Defendance 3 Succorance 2 Compassion 1
When mechanical factors were operating						
Scattered	23	Various	Pain	To improve. To be cautious. To be careful	To protect. To withdraw. To avoid	Harmavoidance 18 Infavoidance 8 Succorance 6 Nurturance 6 Defendance 5 Dominance 4 Autonomy 4 Achievement 4 Blamavoidance 3 Acquisition 2

Table 8. *Degree of recovery in experimental and control group, N = 112*

Degree of recovery	Experimental group	Control group
In <i>status quo</i> or no improvement (0)	8	18
Slightly improved (1-)	4	6
Improved (1)	14	26
Much improved (1+)	20	26
Free from pain or cured (2)	66	36
Totals	112	112

These figures give $\chi^2 = 17.44$; D.F. = 4; $P < 0.01$, with the degree of recovery clearly favouring the experimental group.

The mean recovery times are 25.02 days for the experimental group and 59.54 days for the control group, a difference significantly favouring the experimental group at the level $P < 0.001$.

(3) *The numbers of attendances for treatment after which cure was obtained* are shown in

Table 10, again omitting cases with '0' improvement.

The difference between the mean numbers of attendances (experimental group 8.96, control 18.0) is again significant at the level $P < 0.001$.

INTERPRETATION AND DISCUSSION OF RESULTS

The present investigation has shown that in relation to muscular pains, two kinds of covert behaviour were operating. First, there was covert behaviour which expressed itself in the form of:

(a) 'Protection' of a painful organ or part of the body.

(b) 'Withdrawal' from a painful stimulation existing in the patient himself, e.g. after accidents.

(c) Avoidance of pain.

Secondly, there was covert behaviour that appeared to be an accompaniment of 'problem solving' behaviour, in which free expression to a certain degree was hampered. The patient

Table 9. *Time in which recovery in the experimental and control group took place*

No. of days in which recovery took place	No. of patients in	
	Experimental group	Control group
1- 10	9	2
11- 20	41	5
21- 30	24	11
31- 40	15	17
41- 50	7	8
51- 60	5	15
61- 70	1	9
71- 80	—	7
81- 90	2	3
91-100	—	3
101-110	—	3
111-120	—	2
121-130	—	3
131-140	—	3
141-150	—	3
Totals	104	94

Cases in which there was '0' cure (8 Experimental, 18 Control, see Table 8) are omitted from Table 9.

Table 10. *Number of attendances for treatment after which recovery took place*

No. of attendances	No. of patients in	
	Experimental group	Control group
1- 5	28	2
6-10	44	21
11-15	19	19
16-20	11	22
21-25	1	10
26-30	1	11
31-35	—	5
36-40	—	1
41-45	—	2
46-50	—	1
	104	94

was forced to 'think' rather than 'act' in solving his difficult problem.

In ordinary life we daily encounter many situations in which free expression is restrained. But not all restraint necessarily results in such

a degree of implicit muscular contraction that it becomes pathological. Here arises a fundamental question: when is such covert behaviour most likely to produce ill effects? The answer, on the basis of the present investigation, appears to be: when an individual lives in a prolonged stress situation, produced by the external or internal environment, which he tries to overcome by repeated effort.

Why, then, does an individual not give up his repeated efforts to solve a problem which is far beyond his effective means? A speculative answer to this question was suggested by Snygg & Combs (1949), that behaviour tends to be organized according to the needs of an individual. Any disruption of it is met by continuous efforts to restore and maintain an individually determined relationship between himself and the world about him.

The correlation found between patients' muscular pain and their disruption of behaviour, indicating a fairly close and highly significant relationship between the two, strongly supports the contention that prolonged stress situations are the main factors in evoking localized muscular tensions by way of covert behaviour. The present investigation has revealed a close link between covert behaviour, personality make-up and site of pain. Personality, for the purpose of this research, was conceived as a functional unit and only the outstanding dispositional tendencies which, according to Klein & Krech (1951), determine and qualify the patterning of behaviour were taken into account. Such a conception of personality made it possible to study it in the context of motivational behaviour (Postman, 1953) and to unravel some of the reasons for an individual's overt or covert behaviour.

The evidence obtained suggests that covert motor behaviour is an inseparable part of the dispositional tendencies displayed by the individual. The site of pain (in psychological facilitation) was always in close correspondence, not only with the patient's main reaction to a stress environment, but with the stress situation itself, e.g. pain in the neck was found in ambitious patients who, when confronted

with a rival (environmental stress situation), tried to surpass him by prolonged efforts. It would be interesting to put to test the question of whether the opposite also holds good. That is, whether by knowing the site of pain (muscular structures involved) the patient's pattern of behaviour and environmental stress could be predicted. The stimuli for evoking covert behaviour are manifold, and once pain is initiated various factors operate in sustaining the covert behaviour, in this way reinforcing the pain.

It seems logical to assume then that when once distressing environmental stimuli are removed, the existing covert behaviour should stop and pain cease. But this does not seem to be the fact. A very minor injury, which may have healed completely, may produce pain fluctuations for weeks, if not months, afterwards, or an external environmental stress situation which has ceased to exist, or from which the individual has been withdrawn, may still evoke pain of a fluctuating severity. This can be explained by the fact that covert behaviour can also be elicited by central stimulation, namely by 'self-conditioning' and 'secondary motivation'.

Finally chemical factors may operate in the form of accumulation of waste products as a result of fatigue. These chemical factors may reinforce muscular tension rather than being in themselves initiating factors in evoking covert behaviour.

SUMMARY AND CONCLUSIONS

An investigation of covert behaviour as the causative factor in producing muscular tension, described clinically as muscular rheumatism, fibrositis, etc., was undertaken and a highly significant correlation was found

between these two. Covert behaviour, it is claimed, is evoked by:

(a) Distressing external environmental stimuli which call forth activity in which the patients are not able to express themselves freely and therefore take up a 'postural attitude' towards it.

(b) Stress situations arising in the internal environment; in such cases the covert behaviour takes the form of 'protection' of the painful part, 'avoidance' of pain or 'withdrawal' from a traumatic situation.

It can also be elicited by 'self-conditioning' and 'secondary motivation'.

It is suggested that the accumulation of waste products reinforces muscular tension already present rather than being in itself an initiating factor in evoking covert behaviour.

The relationship between personality make-up, environmental stress and site of pain was examined, and a close link between these three was found. In order to test the hypothesis that muscular pain in its clinical entity constitutes a behavioural disorder, an experimental and a control group were set up. Both groups received similar physical treatment; the experimental group, in addition, received further treatment in the form of 'desensitization'. A comparison of 'cure' of these two groups revealed that:

(a) There was a significantly greater recovery in the experimental group.

(b) The recovery in the experimental group took place in a significantly shorter time and with significantly fewer attendances for treatment.

The conclusion is drawn that the most effective approach used in the treatment of muscular pains was a double one—a combination of simple psychotherapy and physiotherapy.

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THE HUSBAND-WIFE RELATIONSHIP IN CASES OF PUERPERAL BREAKDOWN*

BY PETER LOMAS

Puerperal breakdown and the problems of maternity have been discussed by psychoanalysts almost entirely as phenomena dependent on the intra-psychic conflict present in the mother as a result of her faulty emotional development. This has resulted in neglect of the interpersonal relationships in the present which have a bearing on her illness. In this paper a particular type of husband-wife relationship, which was found to exist to a varying degree in a series of cases of puerperal breakdown, and which seemed to have aetiological significance, will be discussed.

The mothers included in this study were treated by psychoanalytically orientated psychotherapy at the Cassel Hospital where they were in-patients together with their babies (Main, 1958). One limitation of these observations is that they are based mainly on knowledge obtained from the mothers only, for it was the mothers who were treated; their husbands came directly into the therapeutic situation but rarely, although they were encouraged to visit the hospital as much as they liked and to sleep in at weekends. The series consisted of about a dozen mothers treated by the author over a period of two years, as well as a number of similar cases treated by hospital colleagues over the same period of time. This paper describes clinical impressions of dominant features in the husband-wife relationship in these cases, and which appear to have a specific relationship to puerperal breakdown. The mothers' problems have been described here in terms of interpersonal relations. Intra-psychic factors, although of prime importance, have been rather left aside for the purposes of this exposition.

* Manuscript received 15 January 1959.

HUSBAND-WIFE RELATIONSHIP PRIOR TO BREAKDOWN

The total family constellation of these cases was similar to that described by Elizabeth Bott (1957) as 'loose-knit'; that is to say that the links—(geographic, economic and emotional) between present generation and past generation were weak, and the 'family' now consisted of husband, wife and baby, and formed a relatively independent unit. In keeping with Bott's predictions there was no well-defined differentiation of roles between husband and wife. Within such a family structure deviations from a conventional masculine-feminine relationship are more likely to appear.

The most obvious deviation to be seen was a lack of femininity on the part of wife to the extent usually of sexual frigidity; this was replaced by a dominating attitude towards her husband who assumed a passive role. This domination contained elements that could be described as sadistic, phallic and maternal, in varying proportions from case to case. In several instances the domination was masked by an overtly masochistic attitude, but the husband remained the controlled partner just the same.

The concept 'maternal' is not a well-defined one, but if, following Winnicott (1956) it is taken to mean an attitude of devotional care, characterized by empathy and requiring maturity, then these patients cannot be said to have possessed it to any significant degree. Their capacity to dominate and care for their husbands in a way that contained such phallic and sado-masochistic elements could perhaps best be called pseudo-maternal.

Mrs A. was a self-righteous woman with a deeply religious turn of mind and a distaste

for sexuality, who felt herself to be living on a spiritual plane well above that of her husband, of whom she spoke with enormous contempt. He came from a class socially inferior to her own, and suffered from a recurring cerebral tumour; he was well enough to hold a job down but she spoke of him as being irresponsible to the point of imbecility.

Mrs D. was an ambitious career-girl who had always tried to rival men at their own game. She was almost totally frigid but spoke of her marriage as having been satisfactory in the past. She thought her husband unambitious and effeminate—a withdrawn man who pottered about the garden, did what he was told, and submitted meekly to her nagging. He wrote the therapist a gentle and humble letter, in which his own wishes were not in evidence.

Mrs L. was one of the few mothers in the series who had a history of psychiatric illness prior to childbirth. She had been a midwife who had resorted to drugs in times of anxiety about her responsibilities at work. She was a quiet, withdrawn, masochistic woman, who felt herself to be deprived, and who, according to the psychologist's report 'can maintain quite good relationships with people who she can feel deprived like herself and to whom she can be of use'. The man she married was of the type one would expect of her: a quiet, lonely man who 'was not like most men who meet in pubs or take an interest in sport', and who himself had had a nervous breakdown.

The impression gained from this series was that it was not only in the wife's phantasies that the husband was ineffectual and needy, but that her wish for such a mate had influenced her choice of selection in reality. A surprisingly high proportion of the husbands were physically sick men, and several of the wives had been professional nurses.

In spite of its pathological qualities this type of liason was stable up to the time of childbirth, presumably because both parties were receiving considerable gratification from it. In so far as wife was concerned this gratification could be explained not only by the

sado-masochistic elements but by the narcissistic satisfaction involved.

According to Winnicott (1954) the predominant phantasy in narcissistic states is that of the self being cared for by the mother with whom there is identification. These women could indulge in the phantasy of being the phallic mother who looked after the genitally deprived husband/child with whom she was also identified. As the hostility present was bound by the sado-masochistic element the relationship remained a closed system. It was ultimately unsatisfactory, however, for nearly all these patients desired real babies.

Owing to the fact that the husbands personality was not actually studied at first hand, his contribution to this set-up will not be discussed here; it is, however, important to note his passivity and dependence on his wife.

REASONS FOR BREAKDOWN

The introduction of a baby into this system had disastrous results, and it is necessary to consider why an adjustment to the baby could not be made. Many factors, qualitative and quantitative will decide the result in any particular case, but there would in general seem to be several possible outcomes to such a situation.

(1) The wife could transfer her dominantly maternal attitude onto the baby and leave the husband to his own resources.

(2) The wife could adopt a maternal attitude to both husband and baby.

(3) A change could occur in the family configuration in which the wife gained real satisfaction from real motherhood and the husband adopted a supporting role.

(4) Puerperal breakdown could occur.

To take these in order:

(1) In her paper 'Motherhood and sexuality' (1933), Helene Deutsch describes women, whom she calls 'professional mothers', who have successfully pursued this course. In her view, although difficulties of sexuality and of motherhood usually go hand in hand, the frigid woman often being sterile, there is

frequently a split of varying intensity between sexuality and motherhood. She accounts for this split by saying that it is based on the denial of the loved mother as a person who would be so unfaithful as to have a sexual relationship with the father. The methods such women may adopt to maintain this split are described by her as:

- (a) flight into homosexuality;
- (b) the gaining of such masochistic satisfaction from motherhood that sexuality is unimportant;
- (c) variations of the parthenogenetic phantasy.

The last of these two mechanisms are relevant here. They are both based on a narcissistic phantasy similar to the one discussed, but the phantasy involves a real child and emphasis is laid on exclusion of the male. Deutsch describes women who adopt motherhood as a career to the exclusion of all else. The mothers in this series could very well have pursued the same path: they were keen to avoid sexuality, they gratified themselves masochistically to compensate for this, and some of them had parthenogenetic phantasies. (Mrs A., for instance, obsessively perused gynaecological textbooks in the hope of finding that the sperm made no contribution to the growth of the ovum), and they were keen to have many babies, being little discouraged even by their puerperal illness.

It is difficult to understand at first sight why they did not, in fact, become 'career' mothers, but several factors acted against this. Some of these will be discussed later, for they concern the reasons why these patients did in fact break down; they centre on the severity of maternal deficiency due to the intensity of sado-masochism. Another factor, present in some of these families, was that the intense emotional relationship and close identificatory bond between husband and wife would have made the independent pursuit of 'careers' difficult to achieve. The contemporary culture pattern may also play a part in that the belief that children should be allowed freedom of expression has made more difficult the position

of the masochistic 'professional' mother, who possessively dominates her children without compunction; these mothers were very alive to this particular responsibility towards their babies, a responsibility which they found very troublesome.

(2) The outcome of a maternal attitude to both husband and baby is unlikely, for the reason that there are practical and emotional limits to a woman's capacity to care for others if based on masochism, and this limit is likely to be reached during the puerperium more than at any time in her life.

(3) A change in the family configuration with a favourable outcome would be possible if both husband and wife received such gratification and stimulation from parenthood that sado-masochism was lessened and they became capable of greater maturity.

(4) Puerperal breakdown is, of course, this result with which we are concerned. The particular problems that would put a strain on a family such as has been described would be:

(i) The mother is likely to find it difficult to adjust to the demands of a real baby owing to her deficiency in real maternal feeling, and the phallic and sado-masochistic elements in her personality. In the case of a mother who has a marked phallic attitude it can readily be understood that this would cause her to have serious difficulties with her baby; and this could in fact be seen in some of the patients of this series. In the case of the masochistic mother, however, this is not so obvious as there is a widely held view that maternal love is in its very nature masochistic.

Helene Deutsch (1947) has stressed the masochistic nature of motherhood, a view which is in keeping with her concept of 'essential feminine masochism'. There are, however, some reasons for doubting the value of this concept, which is based on the theory that the female sexual desire is a secondary phenomenon, and Winnicott (1947) expresses his disbelief in it. It seems likely then that what these patients lacked was real maternal love which is based not on a masochistic need to

make reparation, but a much more primary phenomenon. The whole question of the essential nature of mother love is, however, beyond the scope of this paper. Nor will it be possible to discuss the diversity of problems which the sexual significance of childbirth and the identifications with the baby, whether boy or girl, will bring to a mother who has manifest severe sexual problems, although such factors were of great aetiological significance in the cases of this series.

(ii) The mother, preoccupied with her baby, and, for reasons outlined above in greater emotional difficulties with him than even a 'normal' mother, has lost her capacity to support her husband and is now urgently needing him to support her. The husband, in his turn, finds new demands made on his self-reliance and on his own capacity to act as a source of strength to his wife, a role for which he has hardly received suitable training. It may be that some of the wife's scorn of her husband, which has been referred to, was on account of his actual failure to fulfil this role. Thus, at a time when her emotional life is disorganized, and she herself has lost her capacity to give support and is herself in desperate need of it, it is not forthcoming.

(iii) The introduction of a third party into a two-party system is always hazardous, but one would expect it to be particularly traumatic to a system that was based on narcissistic gratifications rather than on real object-relationships.

(iv) Owing to the loose-knit family organization, with its social isolation and the unavailability of grandparents and other relatives, the mother was unable to draw on support other than that of her husband.

(v) The problem brought by a changed cultural attitude towards the upbringing of children has already been mentioned. A family in which hostility is avoided by masochistic defences, and spontaneity is lacking, must have found the advent of a lively baby very difficult, especially if duty dictated a forbearing attitude towards the baby's excesses.

THE ACTUAL BREAKDOWN

In the three cases described, the outcome was as follows.

Mrs A. developed a fear that she would kill her baby, became psychotically confused and had to be admitted to an observation ward; after being discharged from there she would return neither to her husband nor to her baby, who was being cared for by her parents-in-law.

Mrs D. kept her baby, but left her husband and went to live with her mother. Her feelings for her baby were, however, anything but loving, and she felt him to be an unpleasant nuisance. When she came to the Cassel Hospital she practically disowned her husband, left the baby as far as possible to others, and expressed a determination never to return home.

Mrs L. did not openly express hostility to her husband or to the baby, but resorted to drug taking to such a degree that threatened the baby's well-being, and was admitted to a mental hospital. The baby was placed in a residential nursery.

These examples were very typical of the series. Sometimes, as in the case of *Mrs D.* the patient developed an open hate of her husband and baby; in other cases hate which was originally concealed by symptoms such as anxiety, was revealed during the course of psychotherapy. A similar emotion of hate to husband and child on the part of the depressed puerperal mother was noted by Zilboorg (1931).

Many reasons could be found to account for the problem which the presence of the baby created for the mother, resulting in the anxiety and hate which she felt towards both husband and child, but one of the main reasons was that she simply could not tolerate the demands which in reality and in phantasy they made on her. She was tormented by the necessity to prove herself a good mother, and had obsessively to do her duty; this is one of the reasons why the masochistic woman is put under such a great strain by the arrival of a child.

That a nursing mother needs to be protected from the sexual demands of her husband is a

fact well recognized in primitive society, which segregates husband and wife during the puerperium and establishes a taboo on sexual intercourse for a varying period, usually up to the time of weaning. These mothers, however, were troubled not only by the sexual needs of their husbands but by their husband's general dependence on them. 'I even have to manage the finances while I'm in hospital', complained one mother.

These mothers could get their much-needed support neither from their husbands nor from anyone else at home and so came to hospital for it. Thus in addition to the intrapsychic conflict engendered in these patients by childbirth and the actual responsibility of a baby, these patients had to bear an additional burden imposed on them by the nature of their relationships with their husbands.

A more detailed description of one of the patients may help to illustrate some of these patterns.

Mrs P., a woman of 31, has been chosen because her real family relationships were more overt than most, and because her symptoms demonstrate most clearly the pressure to which a mother could feel subjected by the changed family situation. In some ways, however, she was not typical of the series: although masochistic and often depressed, she had a more volatile personality than the other mothers, and her symptomatology included more hysterical features; she was the only mother to have been married before; and she proved to have the most intractable illness. In appearance *Mrs P.* was a plump vivacious good-looking woman with a friendly yet somewhat challenging manner. She could remember remarkably little of her childhood, and practically nothing of her father, a waiter, who was regarded by her mother as a social inferior; nor did she remember her mother with any feeling of affection. She had a younger brother whom she now described, with a mixture of admiration and contempt, as a 'Cockney ruffian'. Her adult personality could be understood as having developed out of a need to escape from the family, to feel superior to it in

terms of professional success and cultural achievement, and to compete with males.

Her first marriage, which took place when she was 20, was a brief and unhappy war-time affair, supervised by her mother-in-law, and came to an end when her husband left her while she was in her second pregnancy. Although she felt devastated by her husband's desertion, she pulled herself together, and brought up two boys by her own efforts in spite of severe financial hardship. 'I was father and mother to them', she said.

This independent, bisexual existence was not without enjoyment but when the eldest boy was 7 she sought a fresh husband. The man she chose was elderly, ailing and disfigured, with a facial palsy and a discharging eye and ear—a pathetic figure who, on his own admission (later, to the therapist) had never thought himself worthy of a woman's love. He was a foreman at the works where she was the boss's secretary, and she felt, as her own mother had, that she had married beneath her. Consciously she married him to gain financial security, but her choice was unconsciously directed by a need to have an abject partner to whom she could feel superior, and through whom she could gain her long-awaited revenge on the male—represented in the past by the father who took little notice of her, the virile brother whom she envied, and the husband who deserted her.

The marriage worked satisfactorily for a while, but her triumph was brief, for to her utter surprise and dismay she conceived another child. Her contempt for her husband's virility had been so great that she had never believed he could be fertile and had not taken the necessary precautions to prevent conception. (One cannot help feeling that a need to punish herself for her revenge lay behind this foolhardy attitude, quite apart from an unconscious wish for another child.)

After having the baby (a boy) she became filled with anxiety and was unable to remain at home, moving from house to house, sometimes with the baby or sometimes without, and was sent to a mental hospital where she was treated

by electro-convulsive therapy without improvement. She was finally admitted to the Cassel Hospital with her baby, in a state of the most desperate anxiety. The picture she painted was akin to the nightmare of the Sorcerer's Apprentice: her husband, her two other children, the washing up, the nappies and the baby's crying tormented her. She was tremendously impatient with the baby and felt he should immediately do whatever she wanted.

The type of transference that she developed towards her (male) therapist was such as would be expected of a frigid woman with an envy of men—she showed the greatest contempt for the therapist's interpretations, forgetting them as soon as she was out of the room and became locked with him in an endless struggle for ascendancy. It was this transference that made her case so difficult to treat, and it is likely that a similar situation would have occurred in treatment at any period of her life.

What we are most concerned with here are the problems caused by the birth of the child. The important ones, revealed during the course of a year's treatment, can be briefly categorized as follows.

(1) Problems caused by identification with the baby: these included punishment by the super-ego for the vicarious gratification of primitive desires, and a fear of greed and sadism which had been projected on to the baby.

(2) Guilt arising from unconscious masculine success involved in producing a boy.

(3) A breakdown of a previously stable relation to her husband from which she received the gratification of feeling masculine, superior, and maternally giving, and its replacement by a situation in which she finds herself feeling dependent on him (which hurts her pride), at the same time being unable to receive support from him. Thus the acute anxiety, which was her main symptom, and which at times reduced her to a state in which she screamed or talked hysterically and incoherently, seemed to be due to the fact that she felt herself surrounded by tremendous demands which she could not satisfy. Whereas before the childbirth she had been the family leader,

she now could not bear her husband to leave her, and wept helplessly when her child dirtied the house. Her husband was in fact not capable of firmness and she herself had insufficient respect for him to find him a source of strength.

Due to her sorry plight, and the sympathy she aroused in others, she was the only patient in the series to be readmitted. She returned, in a state of desperation, complete with baby and baggage and demanded to be allowed to come back. This could be seen as a combination of separation anxiety, of her desire to control and her fear of passivity, and a flight from her family to whose demands she had no answer. The distress of this woman was not only manipulative technique but was very real and pathetic; she was bitterly ashamed that she could not respond to the demands of motherhood.

In thinking about this mother the question naturally comes to mind; why did she break down now and not when she had her first two babies? One can do no more than speculate, but there was one important difference in the two situations: her relationship with her first husband, as far as one could tell, was of quite a different nature from the typical one described in this paper, for he was an aggressive and independent man who would neither tolerate her bossiness, nor make demands on her maternal capacity.

In this particular husband-wife set-up the phallic elements in the wife's personality and her sadistic and revengeful domination of her husband were very obvious. His contribution to her breakdown consisted in his inability (in reality) to act as a support to her and in his demand (in her phantasy) for reparation, as a result of her sadism against him and triumph over him.

In the cases in this series in which the patient's attitude to her husband, although dominating, contained more apparently positive and maternal elements, the mechanism of breakdown of husband-wife relationship seemed to be not essentially different from that outlined in this last case, but was less conscious. In these cases the sadistic elements in

the relationship had been bound by masochism and idealism, the baby was openly desired, worries about childbirth was denied, and puerperal breakdown came as a terrible surprise and shock, bringing the mother to a state of acute anxiety.

SUMMARY

In this paper puerperal breakdown is considered from the point of view of the husband-

wife relationship before and after breakdown. A particular type of relationship which has been observed to exist with some consistency, and which is thought to have aetiological significance, is described and discussed.

ACKNOWLEDGEMENT

I wish to express my thanks to Dr T. F. Main for his help and encouragement with this work.

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SPONTANEOUS TRAUMATIC REACTIONS TO HYPNOSIS*

BY L. GOLDIE

Few people have used hypnosis and not witnessed associated anxiety reactions. Mesmer (1948) gave an excellent account of such a reaction which, indeed, he considered a prerequisite of cure, and subsequent reports have been limited to their occurrence in a clinical setting. They have received little attention otherwise although hypnosis has been widely used in the laboratory (Gorton, 1949; Weitzenhoffer, 1953). This may be the result of a false distinction between the clinical and the laboratory situation, workers in the latter failing to report reactions which they feel are not their concern. The following observations were made during a pilot study of the effects, physiological and psychological, of using hypnosis. The object of this study was to indicate those measures and experimental designs most likely to make possible a physiological definition of emotional changes occurring in the hypnotic state. The psychological material arising is presented here for its intrinsic interest and to raise for discussion some ethical, theoretical and practical issues. The present work was preceded by several years experience of using hypnosis in clinical and experimental situations.

In describing the hypnotic state we must distinguish deliberately evoked responses from those that occur spontaneously as a reaction to being hypnotized (Braid, 1846; Freud, 1922*a*; Goldie, 1956). It is a simple matter to differentiate between a direct suggestion that the heart rate will increase, and suggesting an emotion that produces an increased heart rate. In fact we cannot be sure that the tachycardia is the result of being hypnotized, the emotion that this arouses, or the result of our interventions in the form of suggestion. Furthermore, the attempt to hypnotize is an implicit

form of suggestion. Internal psychological factors must determine the result of this form of suggestion. It is not surprising, therefore, to find that we lack a satisfactory description of the hypnotic state (Forel, 1906; Schilder, 1956; True & Stephenson, 1951; Sears, 1932).

There is a diversity of hypnotists, situations and subjects, and the problem is further complicated by authors (Eysenck, 1947; Das, 1958; Ingham, 1954) who fail to ascertain and describe subjective reactions. It is thus difficult to repeat and compare experiments. Frequently 'levels' or 'stages' are described, in each of which stimulus-response association is to be expected; to do this is to ignore the subjective factors that determine the way in which a stimulus is perceived. The following account of traumatic and other reactions lends emphasis to the importance of internal psychological factors in determining responses to hypnosis.

SUBJECTS AND METHODS

Ten subjects were used, all but three of whom were women. Apart from three patients the sample was composed of normal people of above average ability and intelligence. Training was given in two stages, the first directed towards producing a hypnotic state in which, with the eyes open and being able to talk, the individual could produce all the phenomena usually associated with hypnosis, including positive and negative hallucinations (Erickson, 1944, 1954). If this stage was achieved then a second stage followed, in which it was proposed, under hypnosis, that following my instructions the subject could become unaware of sensations in all modalities. The suggestion was made that this, in fact, happens piecemeal in normal waking life. For example, it was pointed out that until I mentioned it he was unaware of the sensations arising from contact

* Manuscript received 17 November 1958.

with the shoes on his feet or the clothes on his body. At other times, when engrossed in a book he might fail to hear his name called or fail to hear the footsteps of someone coming close to him. It was proposed that by similar means the subject might become insensible to his external surroundings. As a result he would be unaware of the objective passage of time as indicated by clocks and external events. He would also lose awareness of identity. Suggestions were then given specifying each sense modality, until finally it was proposed that the subject heard only my voice. When I stopped talking a period of silence would ensue during which he would recover full awareness as this might have occurred in his experience. This would be complete shortly after next hearing my voice.

The psychological concomitants of these two processes are the subject of this paper.

Electroencephalograms and polygraph (combined finger plethysmograph, respiration and electroencephalogram) recordings (Ackner & Pampiglione, 1957) were made on all subjects, hypnotized, awake and asleep. 17-Ketosteroid estimations were carried out, with certain interesting results. For example, 17-ketosteroid output was significantly increased following the experience, in the second stage, of the period of unawareness.

RESULTS

Only four subjects reached the second stage of the procedure, and these four did not include any of the three patients. An account of those subjects in whom the requisite conditions were not obtained is given below.

(1) *Patients*

An unmarried female epileptic who had fits associated with the aura of a man following her. During the training period she improved, according to independent accounts. She attained a trance in which suggested anaesthesia could be demonstrated, though she never attained the waking trance and could not talk in the hypnotic state. The results of urine collections had to be discarded. She was so

frequently disturbed that a control run of specimens from an undisturbed period could not be obtained.

Another patient, a married woman, complained mainly of feelings of unreality. She denied falling asleep, even though characteristic changes were evident in the E.E.G., and also that she had been hypnotized. The active trance was not produced, though anaesthesia was demonstrated and the pulse volume appeared to decrease with the suggestion of cold and with the induction of hypnosis. Her anxiety became continuous as she came to hope that hypnosis would help her, at the same time doubting that it ever occurred.

The third patient, an unmarried woman, entered the trance state easily and would immediately and spontaneously abreact traumatic childhood experiences of seduction by her brothers. This was repeated at subsequent sessions, in between which she was agitated and anxious. Before the experiments she had been 'indifferent' and 'unable to feel anything', and this reaction to being hypnotized could not be prevented.

These three patients, despite explanations to the contrary, regarded the sessions as a form of treatment.

(2) *Other subjects unsuitable for experiment*

Three subjects, though hypnotized, were not suitable for the second stage of the experiment. The first attained a deep trance on several occasions, but never the 'active trance' state. On one occasion he recalled a neutral experience from childhood. This hypermnnesia convinced him that painful material was always going to be elicited and subsequently he would appear deeply asleep without response until told to awaken. Another male was able to initiate and terminate a trance on his own by the second session. After one experience of the 'active' trance state he ventilated fears, via a superficially abstract discussion, of the threat to the 'integrity of the individual'. Discussion revealed him to be a precariously balanced individual and he was not used again.

A third subject did not attain a satisfactory trance state. Having seen hypnosis used she attempted to hypnotize a relative and, being frightened by the effects produced, never attempted it again. She was an active homosexual, being the dominant partner in a lasting relationship. According to her, she wanted to prove to herself that she had no reason to fear being hypnotized. Though she became deeply relaxed the phenomena required in the first stage were not produced. At the fourth session this subject spoke of feeling deep anxiety; she said she was 'split into two', wanting to succeed as a subject yet being afraid to 'let go'. At this time she mentioned thinking of me as a 'seductive person'. The next time we met she said that she felt a wave of anxiety and went on to tell me of her homosexual experiences. This session was followed by a nightmare the same night.

(3) *Subjects in whom requisite conditions were obtained*

In the following four cases the 'active trance state' was obtained and later the 'period of unawareness' of the second stage was produced.

The first subject, an intelligent male, attained the 'active trance state' after the first session, and constantly afterwards. At the fourth session the profound trance was deepened. There were indications that when it was suggested that he was immersing his hand in cold followed by hot water there was an actual vasomotor change. After ten minutes, during which he was quite still, the pulse and respiration rate increased and he visibly perspired. His head rolled slowly from side to side. I spoke again at the end of half an hour and in the next five minutes he visibly relaxed. He became still, with decreasing pulse and respiration rates which remained above the prehypnosis level to the end of the recording. Whilst still hypnotized the subject was asked what he had experienced. He found it difficult to put into words what he wished to describe. The first thing he remembered was a 'funny feeling around the mouth'; he pursed his lips

as he tried to describe his sensations. He haltingly said 'it must have been a breast, mother seemed part of me until I could touch things... for a time I could not see. I had a terrible itching of my face, I wanted to scratch but could not... I felt somehow wet and uncomfortable'.

He denied having read any of the psychodynamic literature. The same evening he wrote an account of his experiences during the period of 'unawareness' which included the following:

The experience which stands out in my mind is when I found myself reliving an experience in my early childhood. The word experience very poorly describes the sensation. I felt during the period, as though I had no knowledge of the outside world, and even my body had no real meaning to me, indeed I was not even aware of its boundaries. It was as though I was everything (The whole world was me). My main aim was to seek satisfaction by satisfying a craving which could be satisfied by oral stimulation and sucking. The breast seemed to be part of me. It was not until a later age that I became aware of myself apart from the breast. I seemed to regard the source of food with mixed feelings of love and aggression. I was gradually becoming aware of other sensory stimuli, such as the discomfort of feeling wet and the comfort of feeling dry and warm. My fingers could touch objects which were *not me*, and my sight seemed to become more and more important and objects took on a definite form and meaning for example the sight of mother... food (? can a baby really begin to reason for itself). I was about six months old when I had infantile eczema and I was resultingly fed by the bottle. I am not quite sure whether I was weaned before the eczema or not.

During the trance I was aware at this stage of a feeling of frustration because I was unable to scratch at the irritation. (I have been told that my arms were splinted to prevent me scratching my face.)

The facts the subject mentions were the outcome of his having questioned his mother following the session.

During the next session in which there were external indications of disturbance, he recalled a dream which had followed an E.E.G. recording

in another laboratory. He was angry, feeling that he had been made a fool of as there were people coming and going who could see him asleep. The dream was of being attacked and having an iron rod pushed down his throat. When asked to have experiences that would be connected with the dream he re-experienced the dream. The instructions were repeated and he recalled earlier and earlier incidents of a homosexual character.

The second subject entered a trance in the first two sessions, which she likened to going off to sleep. At the third session she was able to enter and terminate the state spontaneously and she functioned satisfactorily in the active state, with an amnesia for her movements and activity. Up to this time she had doubted her ability to participate in the experiments and thought that 'real' hypnosis had not occurred in the first two sessions. The fifth session included the 'period of unawareness' and apparent disturbance. It was followed by a headache which lasted three hours. She said that she found it hard to describe her experiences but she 'lost identity' and 'all sense of anything', and felt frightened. A short written report and her account at the next meeting the following day enabled some idea to be formed of her subjective reactions. She had left the session very angry and resentful, feeling unable to talk to me at the time. Her written comments were the following:

You told me that I was going to relive a very unpleasant experience. (She assumes that this was suggested). I could not hear your voice any longer, I felt miserable and dejected. Suddenly I became very frightened as I ceased to have any feelings. I could not feel my body, nor did I know who and where I was, I was just something somewhere and I was terrified. Gradually I regained my feelings. I could picture vividly a man who had a terrifying influence upon me, and of whom I was very much afraid. I had the feeling he took advantage of me, seduced me, and I could not stop him from doing so. I had built up an enormous amount of aggression and hate towards him and after a long struggle eventually I was able to break up all my connexions with him. This often came back to my mind as a kind of night-

mare and horrified me. Now I went through the same experience, only very much more vividly, and I feel that you were the only person that could stop me and you didn't. You let me down and I projected all my feelings which I had towards that man on to you. When I woke up I wanted nothing but to get away from you and I did not want to see you ever again. Afterwards I had a splitting headache and only after about three hours was I able to pull myself together again.

After every session she had a headache. In a session before the last series of urine collections she recounted some dreams she had had and seemed disturbed. She had apparently believed that all her troubles had been resolved by the traumatic session and its discussion. These dreams, which were unpleasant, indicated to her that this was not so. She felt that she could not attend again and did not.

At the first session the third subject had a spontaneous dream of passionately kissing a man who became her father, and saw, with eyes closed, a red-coloured light with a green centre that she had never seen before. She did not want to awaken and did so only slowly. At the second session, in which the active trance state was produced, she was able to move around and talk. Then followed a post-hypnotic amnesia for all that had occurred. While hypnotized she had a pleasant experience that she chose not to tell me about. In addition she relived vividly an incident that had occurred when at the age of four she had been given a doll by her father, her first possession, which had given her great pleasure. These experiences were to her 'actually happening', she was 'in them', and they occupied a great length of time. The subject was now able to initiate and terminate the trance herself and at the third session, whilst an E.E.G. recording was being made, she was given the suggestions for the 'period of unawareness'. At first she appeared to be in normal sleep with the mouth slightly open as if about to snore, and somewhat restless. At the signal to commence the experience she became restless, breathing rapidly and giving the appearance of great distress as she writhed and moaned slightly.

Still in the trance, she told me of her experiences. First, she had felt hungry and had had a pain in her ear and exclaimed 'I must have been a baby'. She was depressed and began weeping. She said 'my mother has just died' in a way that made me think that I had unfortunately timed my experiment to coincide with a real external event, and when I asked her if this was recent she said 'No, when I was ten'. Using the present tense she said that her mother did not love her; mother did not love her because she had been nasty and rude. When the mother was dying she was the only one of the children not recognized by her. She was so distressed that I gave her the suggestion that after she awakened she would forget everything, recalling only what she felt that she could tolerate. She did not have an amnesia for this session and remembered the experiences. She was deeply depressed and disturbed, with insomnia and uncontrollable weeping for the next two weeks, gradually abating. During this time she hated me for having caused this.

I learned later that she had not collected the urine specimens required during this period. As it was undesirable to repeat the traumatic experience it proved impossible to obtain a series of urine specimens covering the 'period of unawareness'. This subject was the most severely disturbed of those experimented upon, having symptoms indistinguishable from those of a depressive illness. Several features require emphasis in view of the common belief that the phenomena appearing as a result of hypnosis are controlled by explicit suggestion. First, whilst in the trance the subject was asked if she wished consciously to remember her experiences and she signified that she did. Secondly, believing as I did then that hypnosis is a state controlled by suggestion, I suggested that she would recall only what she could tolerate. Her reactions showed that this suggestion was not effective and my reliance on it would have led to disastrous results. Thirdly, close questioning might have made the depth of this subject's depression more obvious. For the purposes of physiological investigation it

would have been satisfactory for the subject merely to say the experience was 'vivid', 'pleasant', 'sad', etc. This and the next case show that further inquiry is necessary, and its omission dangerous.

At the first meeting with the fourth subject a trance was obtained. Although suggestions did not produce the active trance state, it occurred spontaneously later. The second session was characterized by a marked tendency to misinterpret. When told to recall a pleasant experience she awakened saying that she had an unpleasant experience that she could not tolerate. Later in the same session she produced consistent responses to suggestions in the active trance state. At the third session the 'period of unawareness' was suggested and after a short time she awakened spontaneously having had 'an extremely unpleasant experience'. She would not enlarge on this. Later I learned that she had relived in a vivid manner an incident occurring in late adolescence. The incident had never been forgotten and she had talked about it to a close friend, apparently without affect. Intense depression with uncontrollable weeping ensued, gradually subsiding over the next two weeks. Later I learned that the episode had been when, at the point of a gun she was raped and threatened with death. Immediately after this the man killed himself.

SUMMARY OF RESULTS

Of ten subjects three were patients and seven were 'normal' and hypnosis occurred in all to a varying degree, being least successful with the patients. An active trance state occurred in six subjects, and consistently in those four in whom the suggested 'state of unawareness' resulted in traumatic and painful reactions.

For two patients, three non-patients and one of those who had severe reactions the induction of hypnosis was an anxiety-provoking procedure.

Seven recounted feelings of being overwhelmed or controlled; in six of these a sexual seduction or rape was the explicit preoccupation.

The one patient who improved never went beyond a light trance with demonstrable suggested anaesthesia, this being associated with a decrease in anxiety. Two patients with increasing anxiety and three normal subjects were unable consistently to reach deep trance states.

None of the four who experienced a severe reaction to suggested unawareness in a deep trance wished to repeat it nor was it thought correct to attempt to do so. The incidents related were sexual seduction or rape in two, babyhood experiences with emotion in one, and in another a childhood experience with depressive anxiety. In this last case hypnosis was at first associated with pleasurable erotic phantasies yet the 'period of unawareness' that was so disturbing had no obvious connexion with sexual, erotic or seduction material.

DISCUSSION

The first aim of this series of experiments was by psychological means, using hypnosis, to produce measurable physiological changes. There are indisputable psychological effects of using hypnosis which hinder the progress towards this goal and it is these that we have here to consider. With hypnosis, as with drug experiments, there are subjective reactions to the situation. The present interest in drug reactions and 'placebo reactors' makes it very necessary to study the psychological situation without drugs. One way such experiments are done is to eschew any way of obtaining real knowledge of the patient's reactions. There are dangers in doing this, especially as psychopathology is more easily ignored than somatic pathology.

In the relationship covered by the term hypnosis there are strong and violent reactions to the experimental situation. In these cases urine specimens were collected incorrectly or forgotten, one result of angry feelings towards the observer. The reactions interfere with the management and detection of dangerous sequelae. These sequelae also occur in drug experiments, as brought out in a clinical study

of the effects of lysergic acid diethylamide by Anderson & Rawnsley (1954). They describe effects strikingly similar to those mentioned here. Amongst other things they mention the prevalence of circumoral paraesthesia, spatial insularity, subjects being 'cut off' from their surroundings or 'remote', spontaneous mood fluctuations, especially anxiety at being left alone, and sudden and prolonged changes in one subject, anxiety changing into more prolonged depression. Sexual excitement was provoked in eight subjects, in one verging on orgasm and in another sexual feeling was compounded with anxiety amounting almost to panic. Paranoid features occurred in eight subjects and there was a disturbance of ego experience in fifteen. They conclude 'It is our impression that there is a rather complex relationship between the mental state under lysergic acid and the surrounding environment'.

It could be assumed that, for the purposes of obtaining quantifiable physiological data, it was not necessary to inquire into the subjective reactions of participants. This present work shows, however, that the existence of subjective reactions should be known to the observer even if it is difficult to elicit them. Information may not be readily forthcoming if the subject is angry and a profound state of depression may pass unnoticed.

A third effect of these reactions to hypnosis is that it makes difficult the repetition of experiments in accordance with accepted scientific criteria. For example, to have any scientific validity the interesting physiological observations made during the traumatic reactions to the 'period of unawareness' would have to be repeated on the same subjects and on others. However, it was not possible in this study to recreate the painful situations, nor was it thought wise to attempt to do so.

This leads to a consideration of the ethical issues raised. Obviously subjective reactions cannot be ignored and we are bound to attempt to anticipate them. The dubious distinction between 'patients' and 'normals' does not help us to do this. We must, in order to be responsible, get personal and confidential

data from subjects, both patients and normals, before and during the experiment. The observer is therefore bound by conditions of professional secrecy and is obliged to do his work in a centre where psychotherapeutic help is available, together with facilities for dealing with psychological sequelae such as those described.

All the patients made an investment in the work as a form of treatment, in spite of explanations to the contrary. Whatever might justifiably be done to patients it is doubtful if they can ever be considered as volunteers, as subjects capable of making the same choice that the 'normal' 'control' or 'non-patient' makes. Our decision to use patients in experiments must be made on the assumption that they have no real choice in the matter.

THEORETICAL CONSIDERATIONS

In many experimental attempts at objectivity using hypnosis, suggestions and external controls are used *as if they alone, interacting with different constitutions, determine the results* (Eysenck, 1947). The results obtained in the present work show that in what is presumed to be a highly suggestible state experiences and reactions are influenced more by internal psychological factors than by any specific suggestions given.

What is often ignored is the 'suggestion' implied in attempting hypnosis at all. The situation is perceived in a special way by the subject. In this small sample, the experiences recounted above were not deliberately evoked and could only have been suggested by the situation itself. Here, hypnosis is equated with being overwhelmed, most commonly a seduction experience. If anaesthesia, amnesia, hypermnesia, etc., are truly the result of our ministrations in the form of suggestions and are acceptable manifestations of suggestibility, we have still to explain other things that did not occur when suggested, and instead resulted in traumatic and other reactions.

The suggested loss of awareness of external stimuli may be considered as a sensory deprivation experiment produced by suggestion. Sensory deprivation experiments as usually

carried out may involve situations which themselves suggest the reactions which may occur.

The explanations of hypnotic behaviour in terms of role-taking theory by Sarbin & Rosenberg (1950) is substantially in accord with the author's experience. It provides a link between hypnotic behaviour and other clinical phenomena from which testable hypothesis may be derived. This explanation in terms of role-taking theory is, however, not an alternative but a complement to the partial explanation given below.

In hypnosis one member of a group of two goes through the actions of controlling another by verbal means alone (Freud, 1922b). The hypnotist is perceived as someone who controls by magic, for by no other means is control without physical contact possible. The other member acts accordingly. These are the manifestations of hypnosis. In so far as these manifestations vary from one individual to another, or in the same individual from time to time, we are seeing the operation of psychodynamic mechanisms in different circumstances and at different times. Unfortunately, the hypnotist, going through the motions as if he believed in magic and thereby suggesting that he does, is prone to see himself (with varying degrees of sophistication) in this way. The subject's reactions to the 'period of unawareness' are to what he thinks might be the result of such a state. A suggestion has been defined (Drever, 1952) as a 'mental process which results in uncritical acceptance, and realization, in act or belief, of ideas arising in the mind, as the effect of the words, attitudes, or acts of another person, or other persons, or, under certain conditions, dependent on processes in the individual's own mind'. We may define suggestion as a mental process which results in the individual, by his acts, beliefs, or attitudes, indicating that he believes that the acts, words, or attitudes of another person can affect him, i.e. magically. The emphasis is now upon the dynamic make-up of the individual affected by hypnosis or suggestion. In situations where drugs or other procedures are used for experimental purposes,

this should be thought of and more account taken of what the individual brings to such situations. The subject, as has been shown, is predisposed by his past life to experience violent emotions and phantasies as a result of the transference that is always made to the experimenter. It is suggested here that transference reactions may be vigorous and are often incorrectly attributed to drugs or the experimenter's intended stimuli. In these circumstances we hardly increase our knowledge of the psychology of the subjects, learning still less about the things that affect them.

SUMMARY

An account is given of the reactions of ten subjects during experiments with hypnosis. Traumatic experiences occurred spontaneously during a suggested state of insensibility to external surroundings, time and identity. The problem of managing these sequelae is discussed along with some of the theoretical,

practical and ethical issues raised. The relationship entered into when hypnosis was used had advantages and also disadvantages that interfered with the planned physiological observations. The observations made in subjects having a severe traumatic reaction could not be repeated. The three patients all made a personal investment in the experiments as a form of treatment, showing that however honest the experimenter is in explaining the procedure and its bearing on their illness patients cannot be considered simply as volunteers for a scientific investigation. The findings show that in what is presumed to be a highly suggestible state experiences and reactions are determined more by internal psychological factors than by any suggestions given.

ACKNOWLEDGEMENTS

I wish to thank Dr B. Ackner and Dr G. Pampiglione for their interest and the facilities they provided.

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INCENTIVES WITH PARANOID AND NON-PARANOID SCHIZOPHRENICS IN A WORKSHOP*

By N. O'CONNOR AND K. RAWNSLEY

FIRST EXPERIMENT

A factory workshop was established in a mental hospital after preliminary work (O'Connor, Heron & Carstairs, 1956) had revealed the capacity of chronic schizophrenic patients to carry out a simple sorting task for long training periods. This unit consisted first of six patients. Numbers were increased gradually to fifty, twenty-five male and twenty-five female patients, all chronic, and, on the male side, all schizophrenic. A description of the problems contingent on establishing a workshop in a mental hospital has been given elsewhere (Carstairs, O'Connor & Rawnsley, 1956). Subcontracted packing work was used in the experiment reported below as a basis for studying learning curves.

Previous work with chronic paranoid and catatonic schizophrenics (O'Connor & Topping, 1959) showed that the paranoid group performed worse when offered a reward, than when not offered this incentive. Another experiment showed that self-competition and a money reward did not increase the level of performance on a mechanical repetitive task with paranoid patients (O'Connor *et al.* 1956). A feature of work curves with patients in both these studies was their slow growth and prolonged steady acceleration as compared with the normal pattern of rapid initial acceleration followed by levelling-off.

No alteration in motive-incentive conditions seemed to change either the trial-to-trial increments ('a') or lead to any sudden change in level ('b'). (Curves were fitted on the simple regression principle described in previous work (O'Connor & Claridge, 1955).) The present experiment was the first systematic attempt to compare two chronic paranoid

groups working on the same task for the same financial rewards, one group receiving the additional stimuli of personal encouragement, knowledge of their scores, and private interviews, while the other did not.

Method

Two groups of ten male paranoid schizophrenic patients were matched for age and intelligence (Progressive Matrices I.Q. and Mill Hill Vocabulary). Mean ages were in the experimental group 44.2 years and in the control 46.6 years, with s.d.'s of 7.11 and 4.54 years, respectively. Matrices I.Q. equivalents were $\bar{X}$ 77.6, s.d. 21.2 and $\bar{X}$ 75.9, s.d. 14.6 for the same groups. Mill Hill I.Q.'s were $\bar{X}$ 77.6, s.d. 9.4 and $\bar{X}$ 77.0, s.d. 6.8. The groups were also matched over two trials for performance on an industrial task, clutch-plate packing. The mean scores for these groups are set out below.

At the beginning of the experiment patients were tested on the Lorr scale (1953), from which various scores could be obtained. Lorr morbidity score was used as a criterion for matching the groups. Mean scores for the experimental and control group were 30.3, s.d. 7.9, and 28.6, s.d. 7.8.

Both groups were given a trial consisting of 20 min. continuous work each day on the packing task, for forty trials. At other times they continued with other work of an industrial nature, but the particular task on which work estimates were based was reserved for this daily period. Daily scores, i.e. units of work completed in a trial, were recorded and graphed. The experimental group was given encouragement during each session, E going to each person and giving him verbal encouragement, urging faster work and saying the previous work had been very satisfactory.

* Manuscript received 27 October 1958.

In addition, before each session, E showed each individual his work curve (after the first day's matching trials), and suggested that he was likely to improve on this during the current trial. A feature of the workshop was a monetary payment made equally to each group.

Patients in the experimental group were interviewed once a week individually for about 30 min. by one of us (K. R.). The aim of these interviews was threefold: (1) to extend the support and encouragement already being given to the experimental group patients in the work situation; (2) to invite disclosure of real or imaginary grievances experienced in relation to factory life in particular and hospital existence in general, and thereby to promote a beneficial reduction of tension; (3) to make the patient aware of his impact upon others with the object of reducing behaviour likely to attract undesirable attention, e.g. loud muttering and gesticulations, and reiteration of bizarre delusions.

Time sampling of psychotic incidents was carried out during each trial. On the basis of randomly ordered observations of patients, behaviour was recorded under three main headings; (E noted if S was working or not, whether he was indulging in any bizarre behaviour and whether he was talking). This latter observation was subdivided according to whether S was talking to himself or another patient. Talking to himself was not included in the category of bizarre behaviour.

The Lorr scale and the two intelligence measures were repeated at the end of the work period. Mean output scores for each group for each trial were calculated and curves fitted. The slopes (*a*) and starting levels (*b*) for each group could then be compared. A full discussion of the technique has been given elsewhere (O'Connor & Claridge, 1955).

Employment in the factory did not necessarily preclude other forms of treatment being administered to the patients. Thus, three patients from the experimental and one from the control group were taking chlorpromazine regularly for some time prior to their contact with the factory and continued to take the drug

during the whole of the experimental period. One patient in the control group was receiving a regular dose of phenobarbitone. One patient in the experimental group and two in the control group began taking chlorpromazine during the course of the experiment. Also, in the experimental period, one patient in the control group was taken off chlorpromazine. This leaves six patients in the experimental group and five patients in the control group who had no contact with drugs whatever during the experiment.

Results

Results of the work curves showed the expected slow development which had been found to characterize schizophrenic performance on this kind of motor task. Over forty trials the mean trial-to-trial increment was just over one unit of output per hour. The matched mean starting level was 20 in the case of the experimental group and 25 in the case of the control group. Final mean levels achieved were 68 and 77, respectively.

Pre- and post-treatment changes

(1) Starting levels for each of the ten Ss in each group compared by *t*-test yielded a *t* of 1.27 which is not significant. Groups were therefore matched on starting trials.

(2) Slope, judged by the coefficient *a* was compared in the same way. A *t* of 0.542 was obtained. The two groups were therefore not significantly different in the rate of growth of the curves. This result was held to affirm the null hypothesis, namely, that encouragement had no effect on the increase in the performance of paranoid schizophrenics. Figure 1 presents the Vincent curves of the two groups and theoretical curves based on their respective coefficients of slope and level. Straight lines were adequate to comprehend the data. The control group appears as slightly but not significantly better than the experimental group.

(3) Time sampling of talk either to self or others yielded practically no positive entries. It therefore could not serve as a measure of change. Bizarre incidents, however, were

recorded and assessed individually for the first thirteen, middle fourteen and last thirteen trials for each group. They yielded means of 5.9 and 7.2 incidents for the first and third subperiods for the experimental group and 4.7 and 1.4 incidents for the same periods for the control group. In the case of the experimental group t (0.685) was not significant. In

from item scores of an assumed normal person.*

Table 1 shows the mean Lorr morbidity score and mean Lorr social impact score for the control and experimental groups at the beginning and end of the experimental period. Reduction of the score indicates clinical improvement.

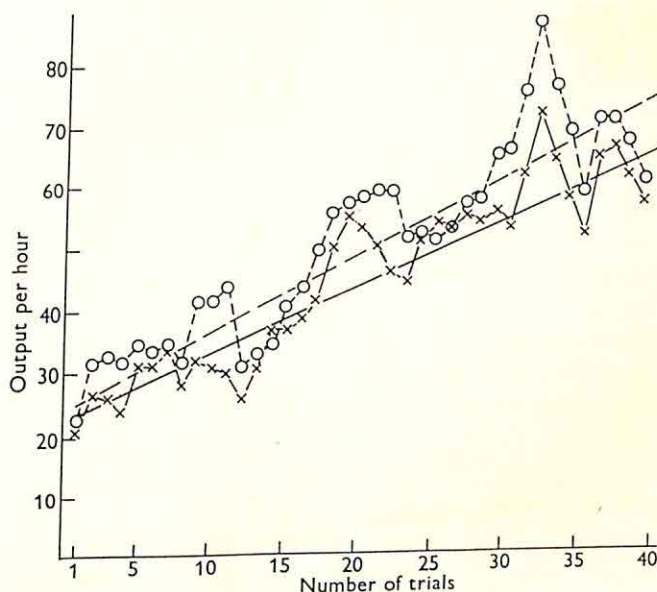


Fig. 1. Daily output per hour for experimental and control groups. Key: Groups Vincent curves slope; experimental, $\times - \times$, — —; control $o - o$, — —.

the case of the control group t (2.644) was significant between the 0.05 and 0.02 levels of confidence. Bizarre activity on the control group therefore decreased in amount during the period of the trials.

(4) Other measures of behaviour were derived from the Lorr scale. Two scores were extracted from each Lorr rating: (1) the morbidity score as defined by Lorr in the description of the scale, is an overall measure of the degree of deviation of raw factor scores from factor scores of an assumed normal person; (2) eighteen items were selected from the Lorr scale with the object of deriving a 'social impact' factor, that is, a measure of those facets of behaviour most likely to be immediately evident to another person or a social group in everyday contact. The factor is the sum of the deviations of raw item scores

Table 2 embodies a comparison of individual morbidity and social impact scores at the beginning and end of the experimental period. Both in the control and experimental groups the morbidity score changes in the direction of normality but the change is insignificant. There is a difference between the groups, however, in respect of change in social impact score. The experimental group shows an insignificant change for the worse, whereas the control group score changes in the direction of normality by a significant amount.

(5) I.Q. scores for each group were not significantly different at the commencement of the trials. The change in matrices I.Q. scores (Table 3) were significant for each group providing the assumption is made that the same

* See appendix for details of the items used.

mean score would be expected. However, this is probably an unjustified assumption. In general, if we follow Vernon & Parry (1949), an I.Q. rise of five points might be expected as a maximum mean change. The gain of ten points, in one case, and eight in the other, is therefore greater than maximum expectation or practice effect. In terms of individual changes two were negative (two points in each case) and three zero. Fifteen were therefore positive. Without a control group and in view of the lack of change in the Mill Hill Vocabulary, it is impossible to say whether this

Relationship between measures

(1) Changes in the Lorr scores used in this experiment were not significantly related to output changes. (ρ for the 'social impact score' changes and output changes was 0.03 and for the morbidity score and output changes 0.246. Pearson's conversion yields product moment correlations of 0.03 and 0.257 which are not significant.) At the same time it must be noted that the relationship between the morbidity score and the initial (matching) performance was not significant ($\rho = 0.03$).

Table 1. *Means and standard deviations of the Lorr scale ratings of morbidity and social impact scores before and after training*

	Lorr morbidity score		Lorr social impact score	
	Control group	Experimental group	Control group	Experimental group
Before training				
Mean	28.60	30.30	11.30	11.50
S.D.	7.76	7.87	2.65	5.87
After training				
Mean	26.40	28.40	9.60	11.60
S.D.	9.60	8.51	2.11	4.03

Table 2. *Lorr morbidity and social impact scores. Comparison of individual scores by t-test at the beginning and end of the experimental period in the two groups of patients*

	Control group			Experimental group		
	Before	After	<i>t</i>	Before	After	<i>t</i>
Lorr morbidity score	28.6 ± 7.2	26.4 ± 11.1	1.40	30.3 ± 9.1	28.4 ± 7.8	1.36
Lorr social impact score	11.3 ± 2.6	9.6 ± 2.3	3.05*	11.5 ± 5.9	11.6 ± 4.3	0.07

* Significant at the 0.02 level.

change is explicable in terms of a treatment effect or not.

The investigators take the view that some of the large changes shown indicate a resuscitation of function which may not be due to practice effect alone. Six of the fifteen positive changes were of sixteen points or greater. Further investigations would be needed to establish this point.

Table 3. *Changes in progressive matrices scores*

	Experimental group		Control group	
	Before training	After training	Before training	After training
Mean	77.60	88.00	75.50	84.20
S.D.	21.22	23.04	14.50	15.15

Apparently therefore the level of patients' work performance was not determined by the acuteness of their mental symptoms.

(2) Initial matrices I.Q. was not significantly correlated with slope ($\rho = 0.21$, $r = 0.22$) nor was final matrices I.Q. significantly related to slope or output improvement ($\rho = 0.30$, $r = 0.31$). As with the Lorr scale, initial matrices score was not related significantly to initial output levels ($\rho = 0.14$, $r = 0.15$).

Discussion

Several features of the results are of great theoretical interest. First, the failure of encouragement to affect the level or slope of the learning curve is notable. Secondly, the long straight-line curve needs explanation, in so far as it reaches a high level of output. Figure 1 illustrates the straight-line growth functions of the two patient groups compared with their smoothed output curves. These two features of the curve will be discussed first. Group curves and their relation to clinical tests are discussed below.

If it is supposed that drive is increasing, then the result expected would be an approximation to a straight line or a positively accelerated growth function. (On the basis of learning theory a high asymptote would imply a large first increment because increments decrease according to Hull (1943).) This result was found. Despite the small increments the level of performance achieved by the fortieth trial is relatively high, indicating perhaps the existence of high drive towards the end of the experiment. In previous experiments (O'Connor *et al.* 1956) it was shown that chronic paranoid schizophrenics took about five times as many trials as normal controls to achieve the level at which normals reach their first plateau on that particular task. The arousal of this increasing drive appears to be evoked by repeated trials in a proportional relationship.

The comparison of the encouragement and the control group on performance shows that the encouragement did not affect the size of the increment significantly. Thus, the increase in drive which is theoretically required to explain

the results, is automatically engendered by the performance of the task and is not assisted by personal contact of an 'encouraging' kind.

The slow development of the curves suggests that long periods of treatment may be necessary with this chronic group. The results are, however, very encouraging in the area in which they have been investigated. Changes in behaviour other than that practised were relatively small, but changes in the practised task were great. It is conceivable that specific social conditioning might be equally effective. This problem of behaviour change is now discussed.

From the psychiatric standpoint, the patients' slow but sustained improvement in performance on the industrial task is not unexpected. It is common for schizophrenia to produce a reduction in drives of all kinds. Furthermore, prolonged contact with the chronic ward culture of the mental hospital, with its absence of obvious goals and lack of stimulation, undoubtedly contributes to the anergia. Therefore, one might expect a learning curve to rise very gradually at the outset. The stimulus of the factory situation involving financial reward may be supposed to vivify drives atrophied through long stay in the institution or damped years ago by the schizophrenic process, now partly receded.

It is more difficult, however, to account for the responses, both negative and positive, of the two groups of patients. On the one hand, they fail to show any differences in their work performance; on the other, an improvement in two measures of abnormality of behaviour was shown by the *controls*, but not by the experimental group. It appears that the patients in the experimental group were not unresponsive to the social stimulation which they were given in this group: they reacted against it. The programme of personal encouragement and of regular private interviews, which was designed to overcome the negativism of these paranoid patients, served only to uncover it, and hence to offset the non-specific therapeutic effect of the work situation. This paradoxical finding is not unprecedented, Miller, Clancy & Cumming (1953) described

similar results when groups of chronic schizophrenic patients were given intensive treatment for a period of eight weeks. At the end of this period, the experimental groups showed significantly more evidence of hallucinations than did the controls; but they also showed a significant improvement during the period in their level of occupation and in their display of interest in their environment.

Both the Miller *et al.* study and the present paper contrast with the numerous reports of the effects of total push programmes with chronic psychotics (Kamman, Lucero, Meyer & Rechtschaffen, 1954) whose records of improvement in patients' mental state have been so uniform as to provoke Lucero to write, after a review of the literature: 'Almost anything that has at one time or another been tried has been of benefit in the treatment of schizophrenia'. The clue to this apparent contrast may lie in the time factor. Both of these have been short-term studies, whereas 'total push' programmes are generally evaluated after periods of six months or a year. A recent paper reporting remarkable progress in chronic schizophrenic patients in response to social therapy (Cameron, Laing & McGhie, 1955) noted that some eight months elapsed before significant changes in the patients' level of social behaviour emerged. In this connexion it is worth mentioning that marked clinical improvement has been noted in certain chronic paranoid patients, not included in this experiment, after they had been in the factory for a year or more. These patients in time became able to communicate with a doctor or a congenial workshop supervisor to a degree which is impossible during the early months of contact. Their behaviour is comparable to that of young children, who will react with alarm if a stranger approaches them effusively, but who will accept the same person if they are allowed some time in which to become familiar with his mere presence.

The implication of the present experiment is, therefore, that chronic paranoid patients will respond better to a régime in which

socially rewarding experiences are made available (but not thrust upon them) than to one in which they are immediately involved in person-to-person relationships.

Summary

Two groups of chronic paranoid schizophrenics matched for age, I.Q. and performance on the task chosen were compared on the basis of this task and their behaviour during the trial sessions. The experimental group was encouraged during each session and interviewed individually once each week. Each member of the group was shown his individual results and urged to surpass his previous efforts. In the control group no encouragement was offered, psychiatric interviews were not given and the patient was not asked to surpass previous scores.

As measured by performance curves taken over the forty trials the groups did not differ significantly. Bizarre behaviour as measured by time sampling improved in the control group but not the experimental group. With a 'social impact' score based on the Lorr scale the control group moved significantly in the direction of normality, but the experimental group showed an insignificant change for the worse. Treatment was therefore ineffective as shown by some measures, and for others it appeared to make the patients worse. The forms of incentive offered did not affect the slope of the straight-line learning curve apparently characteristic of this group of patients.

Results are discussed in terms of psychological and psychiatric theory. It is hypothesized that paranoid patients may respond better to social rewards which are available for their acceptance, rather than to social rewards which they are asked or pressed to accept. It is further suggested that psychologically speaking an explanation for the relatively straight-line learning curve can be given in terms of the pressure of a continuously increasing drive state in this group of patients.

SECOND EXPERIMENT

In the previous experiment learning among paranoid schizophrenics was reported. In this a similar investigation involving chronic non-paranoid schizophrenics is described.

The major interest in the present study was to discover the extent to which the findings of the previous study with chronic paranoid schizophrenics would hold good for chronic non-paranoid patients. It appeared from previous work that the non-paranoid patients would have a different learning curve and might react more positively to the regimen of occupational treatment, involving as it did incentives and encouragement.

Method

Two groups of ten male non-paranoid schizophrenic patients, all of whom had spent more than two years in hospital, were matched for age and verbal intelligence. The mean age of the experimental group was 39.2 ± 8.1 and of the control group 38.6 ± 3.9 years, t for the mean difference (0.17) was not significant. The I.Q., estimated by a progressive matrices, was 68.3 ± 22.4 for the experimental and 77.6 ± 25.0 for the control group. I.Q. estimated by the Mill Hill Vocabulary was 70.6 ± 6.2 for the experimental group and 75.4 ± 6.8 for the control group, t for the difference between the means (1.7) was not significant. The Lorr scale was used to provide a rating of behaviour before and after the experiment. The experimental and control groups were matched initially on the basis of the Lorr morbidity score. Both groups of patients began work in the hospital factory on a packing task identical with the one used in the previous work with paranoid schizophrenics. Their performance was measured for half an hour each day, and the number of items packed during this time was noted for each individual patient. The first two trials were used as a basis for matching the two groups of patients for initial performance on the task.

As in the previous experiment both groups

were paid a piece rate for the work. The experimental groups received, in addition, frequent encouragement from the factory supervisors, and each day were shown their performance score from the previous day's work, and were encouraged to surpass this score. Each member of the experimental group had an interview with one of us (K. R.), once a week. The interview was generally supportive, and an attempt was made to point out to the patient aspects of his behaviour likely to prove detrimental in an ordinary social setting. The patients were also encouraged to voice their feelings and grievances about their hospital life in general and about the factory in particular.

Time sampling of behaviour in the factory was carried out with particular reference to psychotic or bizarre activity. The experiment lasted for a period of 40 days in all.

Results

Table 4 sets out the initial and final scores for the two groups of patients in terms of the number of items of work packed per day. The initial score is based on the average of the first two trials, and the final score on the performance at the fortieth trial. The two groups were, of course, matched in terms of their initial score, and a t -test for the difference between the two groups yields a value $t = 0.60$, which does not approach the 0.05 level of significance. Comparison of the final scores for the two groups also shows that the difference between the groups is insignificant— $t = 0.21$.

Table 4. *Packing task performance*
(items completed per hour)

	Initial score	Final score
Experimental group	28.6 ± 11.3	87.8 ± 49.2
Control group	25.4 ± 11.2	83.2 ± 43.2

Initial scores are based on the average of the first two trials.

Final scores relate to the fortieth trial.

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Inspection of the individual learning curves for both control and experimental groups shows that unlike the majority of the paranoid schizophrenics, non-paranoid patients yield curves which are negatively accelerated and resemble, in general, the logarithmic curve familiar in learning experiments with normal subjects. The composite curve for each group, based on mean scores for each trial, is also negatively accelerated. Under these circumstances, the method of analysis used in the paranoid experiment, which involved fitting a straight line to each of the curves, and comparing the slopes of these lines, was not used.

or bizarre incidents in each period were noted. Over-all, both groups showed a slight tendency to manifest more psychotic behaviour in the middle period, but the number of incidents recorded was very few indeed.

Table 5. *Time sampling of bizarre or psychotic incidents during trials*

	Periods		
	I	II	III
Control group	3	7	3
Experimental group	2	14	8

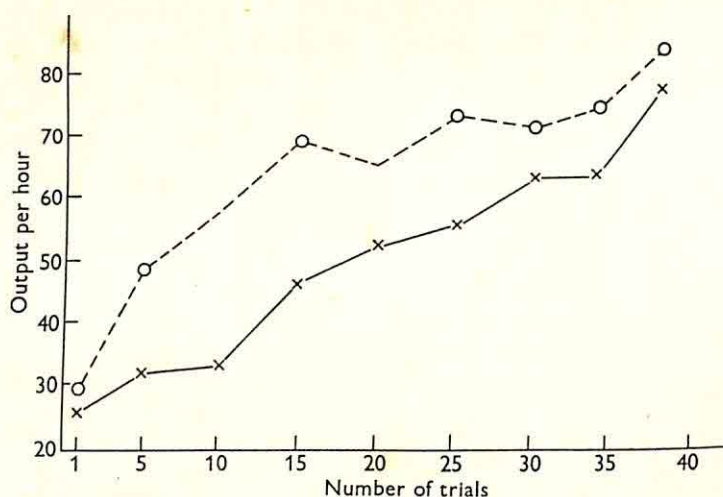


Fig. 2. Comparison of paranoid and non-paranoid schizophrenic smoothed learning curves.
Key: paranoid, $\times - \times$; non-paranoid, $\circ - \circ$.

By inspection of the learning curves from the present experiment, and from the previous work with paranoid patients, it was noted that the non-paranoid patients reached a score approximately double the initial score within twelve trials, whereas the paranoid groups needed eighteen trials to double their starting score. Figure 2 shows the two smooth curves. One for all paranoids in the previous experiment and one for all non-paranoids in this.

The results for the time sampling of psychotic behaviour are set out in Table 5. For purposes of analysis the experimental period was divided into three sections of thirteen, fourteen and thirteen trials, respectively, and the psychotic

Two scores were derived from the Lorr rating in each case, and the results are given in Table 6. The first score is a general rating of deviant behaviour called the morbidity score, and the second is based on those items of the Lorr scale representing aspects of behaviour most likely to be evident to another person or a social group in everyday contact: this was called the 'social impact' score. In terms of both the morbidity score and the 'social impact' score, both the experimental and control groups showed an overall improvement during the course of the experiment. The changes however, were relatively slight, and were not statistically significant.

The relationship between Lorr scores and

Table 6. Mean morbidity score and 'social impact' score derived from the Lorr scale administered before and after the factory experience

	Control group			Experimental group		
	Before	After	<i>t</i>	Before	After	<i>t</i>
Lorr morbidity score	28.1 ± 7.7	26.7 ± 11.2	0.29*	29.7 ± 12.2	28.4 ± 10.5	0.24*
Lorr social impact score	13.6 ± 4.6	10.9 ± 4.1	1.30*	15.8 ± 7.5	12.9 ± 4.2	0.99*

* Not significant.

output was found to be insignificant. Lorr scale score and final output measure correlated -0.086 for the combined groups. Similarly, the relationship between I.Q. and output was insignificantly low, 0.045. This was the correlation between progressive matrices, equivalent I.Q. score and final output score for the two groups.

Discussion

In the learning experiment carried out with paranoid schizophrenics, no difference was found between the performance of the encouraged group and that of the control group. The findings in the present experiment are similar, and it may be concluded that encouragement, interviews, and knowledge of results, do not have any substantial effect in enhancing performance in chronic non-paranoid schizophrenics under the conditions of the experiment. There is, however, a difference between paranoid and non-paranoid patients in respect of the shape of the learning curve. The paranoid groups follow a relatively straight-line course in their acquisition of skill, whereas the non-paranoid patients show an initial steep rise in performance, followed by a gradual flattening of the learning curve, reminiscent of the behaviour of normal subjects. The results imply, in Hullian terms, that the non-paranoid group begin their task with more drive than do the paranoid patients, whose slowly rising straight line curve suggests a progressive arousal of drive.

As with the chronic paranoid patients the considerable improvement shown in performance was not reflected in the clinical state. *Occupation at a factory task for a*

relatively short period evidently does not lead to clinical improvement, even though the patient is given an incentive, encouraged and praised.

The non-paranoid group differ from their paranoid counterparts in that encouragement and interviews do not appear to have a positively retarding effect on clinical improvement. This may conceivably be related to the absence of markedly suspicious and distrustful attitudes in the case of the non-paranoid group.

The investigation shows that considerable improvement in the performance of these patients can be achieved in a short period, but that it is not accompanied by improvement in their social behaviour. This indicates the need for also developing training techniques of a social kind.

In the case of both the experiments reported the mean scores have been presented in discussing learning and clinical improvement. Individual curves and behaviour changes usually vary considerably from person to person. It would need a separate article to discuss the spread of differences in our data, but some idea of starting-level differences and final-level differences in learning scores can be obtained by quoting the ranges of each for the experimental and control groups in each experiment. These were 5-11, 3-13 and 12-72, 32-84 for the two paranoid groups. For the non-paranoid groups the starting ranges were 6-40 and 0-19, and the final levels 16-98 and 3-84, respectively. No full analysis of the exact relationship between starting level and final level has been attempted, nor any analysis of spread on curves. However, there is very con-

siderable score fluctuation in each curve. Some patients momentarily achieve their ultimately highest score as much as ten days before the cessation of the experiment when this score is considerably in excess of their average at that time. Likewise, patients may fail to achieve more than their starting level even as late as the thirty-fifth trial. The upward slope of average performance is achieved despite these fluctuations. There would also appear to be equally large fluctuations in both diagnostic subgroups, but this is a matter for further analysis. The patients who did not follow this pattern of performance were extremely rare. Four only failed to achieve a performance level of 300 % of their starting level. One of these was a paranoid and three were non-paranoids. Their starting levels were not particularly low but their final scores showed little improvement.

Summary

Twenty non-paranoid chronic schizophrenic males were divided into two groups matched for age and performance on a packing task. One group was given treatment and encouragement and goal-type incentives. The other group

was used as a control. Although very considerable improvement took place in the output of both groups, over 300 % in each case during a period of three months, there was no difference in rate of improvement between the groups and no apparent effect of either treatment, encouragement or incentive. As in a previous study with paranoid schizophrenics, there was steady continuous improvement unaffected by the incentives used.

The curves of learning for each group achieved defined stages of improvement over starting level sooner than had been the case with paranoid schizophrenics. Although great improvement took place in performance on the training task, no measurable change in social behaviour could be recorded. It is conceivable that conditioning of social behaviour might prove as successful as the training of skills in a workshop has proved to be with these groups.

ACKNOWLEDGMENTS

Our thanks are due to Dr E. P. H. Charlton, Physician Superintendent of Banstead Hospital, who kindly provided facilities for the experiments reported here. Our thanks are also due to all those who participated.

APPENDIX

Items from the Lorr scale used in constructing a 'social impact' factor. (The numbers quoted are the actual numbers of the items in the Lorr scale)

- | | |
|---|--|
| 1. Relevant speech—irrelevant speech. | 48. Co-operative—unco-operative. |
| 3. Harmonious thought and feeling—disharmonious thought and feeling. | 50. Special help needed to fulfil ordinary habits—no such special help needed. |
| 8. Mute—over-talkative. | 52. Does not reply when addressed—talks excessively in reply. |
| 11. Coherent speech—incoherent speech. | 54. Sensible speech—speech not sensible. |
| 12. No hostility—extreme hostility. | 55. No resistance to expected routine—frequent resistance. |
| 29. No speech blocks—speech blocking. | 56. Never speaks to others—always speaking to others. |
| 31. Restraint in showing feelings—no restraint in showing feelings. | 58. Stays by himself—sociable. |
| 36. Emotionally under-responsive to situations—emotionally over-responsive to situations. | 60. No assaults—assaultive. |
| 47. Rarely irritable—frequently irritable. | 61. No bullying—teases and bullies. |

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REVIEWS

Schizophrenia: A Review of the Syndrome.

Edited by LEOPOLD BELLAK. (Pp. xx + 1010. \$14.75.) New York: Logos Press. 1958.

For the second time Dr Bellak has undertaken the formidable task of bringing together in one volume all the recent developments in the study and treatment of schizophrenia. On this occasion he has invited a number of authorities to contribute from their own special experience. As a result we are presented with what is undoubtedly the most comprehensive work on schizophrenia to appear in English. There is no aspect of the subject which is omitted. Psychological and organic approaches, in both the fields of aetiology and therapy, are given equal representation. Chapters on 'Vital Statistics', on 'Physiological Studies', on 'Psycho-Analytic Aspects', on the 'Shock Therapies' and on the 'Tranquillizing Drugs' are among the subjects which are contained in this book.

This is not a heterogeneous collection of articles written by a number of specialists. In the introductory chapter, which gives an excellent outline of the detail which is to follow, Dr Bellak shows how many apparently divergent views can be reconciled with one another. He proposes a multiple factor psychosomatic theory of schizophrenia whose function is to accommodate both psychological and physiological findings from the clinic and from the laboratory. He suggests that the varied manifestations of schizophrenia must be regarded as a 'final common path' of a number of conditions which become apparent through a severe disturbance of the ego. These conditions range from a mild psychological weakness of the ego to diseases of the ego caused by infection, organic disease or by genetic influences. His belief that the conditions labelled schizophrenia have a multiplicity of causes facilitates co-operative efforts by psychologically and organically orientated psychiatrists.

In a book of this scope, containing as it does such a wealth of clinical and experimental data, it is not easy to select particular topics for detailed discussion and disregard others. The texts contain admirable summaries of all the recent literature. This is, primarily, a book for the specialist in schizophrenia. It is also a book which reflects the

healthy state of American psychiatry—for in it physiological and psychoanalytic studies are not regarded as mutually incompatible, but as complementary and as different facets of the trend towards a final understanding and treatment of the disease.

THOMAS FREEMAN

Peptic Ulcer and Psychoanalysis. By ANGEL GARMA. *Nervous and Mental Disease Monographs* (No. 85). (Pp. 143 + vii. 48s.) London: Baillière, Tindall and Cox. 1958.

In the thirteen chapters of this study Dr Garma has attempted to expound a complete theory of peptic ulcer. He describes the divergent theories of other psychoanalysts who have investigated personality in relation to gastric function (such as Franz Alexander and S. G. Margolin), but devotes most of his book to the statement and repetition of the theory he has developed in the course of his own extensive clinical experience in this field. Evidence from other disciplines (e.g. physiology) is introduced in support of his theory. Dr Garma has been much influenced in his approach to psychopathology by the work of Mrs Klein and her followers, who should not, however, be held responsible for his views on peptic ulcer.

In order to present Dr Garma's essential thesis fairly, we can quote his own words:

A. Examination of ulcer patients leads to the hypothesis that ulcer should be regarded as a self-inflicted digestive bite provoked by guilt on account of oral-digestive desires against the mother's breast. We have seen that the mother's breast acquires importance for the ulcer patient because, regressively, it represents genital objects and that digesting represents the act of coitus. In other words, the superego (the internalized mother) bites the ulcer patient internally. (Bite being an oral expression of digesting internally.) Ch. X, page 109.

B. My theory could be called *the bad internalized mother acting aggressively upon a person in oral-digestive regression*. . . when this person finds himself in situations of conflict, the reactivated unconscious psychic image of the bad internalized mother, acting inside him like a severe, subjecting, illogical and punishing conscience, provokes excessive remorse in

him, which as the word itself implies, is unconsciously based upon sensations and reactions of being bitten internally. They also provoke other internal oral-digestive aggressions of another kind. This psychic image of a bad internalized mother also drives him to alimentary disorders, such as eating little or renouncing certain foods, ingesting food which is indigestible either by itself or because the person psychically endows it with bad qualities, and excessive drinking or smoking. All this acts against his stomach. It gives rise to an increase in chlorhydropeptic secretion, hyperperistalsis, vascular and muscular spasms, diminution of the efficacy of the mucous barrier, lack of certain amino-acids and antacid hormones and other organic disorders which lead to gastric and duodenal ulcer. Summary, pages 136-7.

In these quotations two versions of the theory can be seen in uneasy co-existence. In the first version, a specific unconscious phantasy expresses itself in a symbolic way so as to form a specific lesion in the duodenum or the stomach; it is even implied that the ulcer crater itself, which is said to resemble a nipple in shape, has primary symbolic significance. According to the second version, a specific unconscious phantasy leads to a variety of harmful oral practices which may produce a wide range of *physiological* disorders: it is of these that the outcome may ultimately be an ulcer.

If the second version of the theory is true, then 'the bad internalized mother complex' is one of many factors that may affect appetite and digestion in a manner injurious to health. As for the first version of the theory, it simply does not make psychophysiological sense to say that a lesion in an internal organ of which the average layman has only the vaguest notion, a lesion resulting, as far as is known, from a complex interaction of mechanical, involuntary nervous, hormonal and other chemical processes is 'a self-inflicted digestive bite'. The patient when he talks about his symptom, after he is conscious of his gastric disturbance, may speak *as if* it were the result of a bite, whether spontaneously or under the influence of Dr Garma. But neither secondary elaboration on the part of the patients nor ingenious rationalization on the part of Dr Garma can make it any the less nonsensical as a theory of the mechanisms of peptic ulcer formation. Dr Garma, however, manages to have complete confidence in both versions of his theory at the same time, and recommends deep psychological treatment for every patient with a peptic ulcer.

Dr Garma discusses duodenal ulcer and gastric

ulcer together as if they had a similar aetiology though there is increasing evidence to the contrary. It is now widely accepted that duodenal ulcer is a stress disorder. Psychic conflict is one source of stress, and the psychoanalyst is in a better position than anyone else to deal with the unconscious factors involved in it. Thus, to those patients with duodenal ulcer, whose main stress results from neurotic disorder, the psychoanalyst can probably bring relief, though even in these cases the aid of the physician and the surgeon is often indispensable.

Dr Garma has no doubt been able to do much for his patients, a small selected sample of the millions who suffer from peptic ulcer, by letting them talk about their problems and trying to clarify these. He has included in his monograph an abundance of clinical material showing very vividly how emotional conflicts can affect eating, drinking, smoking and digestive processes. He quotes some interesting and little-known literary illustrations of the relation between the emotions and attitudes to eating and drinking. He has made a contribution which no research worker can afford to neglect, however much Dr Garma's theoretical formulations may provoke disagreement.

VICTOR B. KANTER

Newsletter: Transcultural Research in Mental Health Problems. Edited by E. D. WITTKOWER and JACOB FRIED. Montreal: McGill University (Department of Psychiatry and Department of Sociology and Anthropology). 1958.

This is the fourth of a series of newsletters which were initiated in 1956 'addressed to the international community of psychiatrists and social scientists who are actively engaged in research concerned with the relationship of culture and problems of mental health'. A Section of Transcultural Psychiatric Studies has been formed in the Department of Psychiatry at McGill University in order to further the undertaking. Its sponsors, Profs. Ewen Cameron and Wittkower organized a meeting of interested psychiatrists from many countries during the 1957 Congress of Psychiatry in Zürich. This meeting, which is discussed in the present Newsletter, revealed that neither the editors nor their contributors were very clear in their minds about the precise meaning

of 'Transcultural Psychiatry'. Prof. Ødegård spoke with the authority of many years' experience of research in social psychiatry when he pointed out that two necessary preliminaries to comparisons between psychiatric and cultural observations in different countries are, first, uniformity in the ascertainment of data, and secondly, the establishment of norms for the influence of particular social factors on mental disorders in our several communities. It may be that much more basic work will have to be accomplished in social psychiatry in single communities before cross-cultural comparisons can become meaningful. During this preliminary phase it is both interesting and instructive to read accounts of social-psychiatric research in countries other than one's own; and it is stimulating to exchange ideas about the possible influence of cultural traditions upon psychiatric syndromes. The current Newsletter contains reports—necessarily brief—of research in several continents, and speculations which indicate an abundance of problems still to be explored in this field.

MORRIS CARSTAIRS

Psychopathology: A Source Book. Edited by C. F. REED, I. E. ALEXANDER and S. S. TOMPKINS. (Pp. 803. 100s.) Harvard: University Press; London: Oxford University Press. 1958.

This book consists of a selection of papers by different authors previously published elsewhere, mostly since 1952, and is designed for students of abnormal psychology as a supplement to the standard and systematic textbooks. The editors are three teachers of psychology at Princetown University and their book is in effect a bibliography for their students of suggested reading in the recent periodicals. It consists of five sections followed by an index: Psychopathology and Early Experience; Psychosomatic Disorders and Neurosis; Schizophrenic Psychoses; Somatic Factors in Psychopathology; and Psychopathology and the Social Context. In all, there are forty-six original articles and this review cannot hope to do them justice.

The articles are of three kinds: reviews of a topic by an acknowledged expert, such as that by Selye on 'Stress and Disease', or that by Funkenstein on 'The Physiology of Fear and Anger'; partial expositions of an established approach to

a problem, such as that by Fromm-Reichmann on her 'Psychotherapy of Schizophrenia', or that of Maxwell Jones on his 'Therapeutic Community'; and reports of experiments more or less systematically conceived and performed, such as Shapiro's 'Investigation of the Block Design Rotation E Effect', or part of Hollingshead and Redlich's extensive survey of 'Social Class Differences in Attitudes toward Psychiatry'. The selection also gives the student a fair idea of the wide range of styles of investigator at work in the field of psychopathology—from the rough and tumble of Rosen's report on his 'Direct Analytic Therapy' to the meticulous extrusion of negative results by Witt in 'The Identification of Small Quantities of Hallucinatory Substances in Body Fluids with the Spider Test'.

The editors have tried to make their selection representative of the contemporary field of psychopathological investigation in all respects except that of quality. In this respect, luckily for us, they have tried to include only the best papers. So far as this reviewer knows they have largely succeeded, and it may well be the fault of the limited available material that whereas the 'Psychological Consequences of Brain Lesions and Ablations' are excellently reviewed by Klebanoff, Singer and Wilensky, the article by Himwich on the 'New Psychiatric Drugs' does justice neither to this extensive topic nor to his own renowned skill. The article of Pilot *et al.* on 'Duodenal Ulcer in One of Identical Twins' does perhaps get perilously close to that familiar type of paper in which an interesting problem is given quite inadequate treatment and is larded with suggestive guesses that sound like conclusions. In contrast, the paper by Rosenthal and Frank on 'Psychotherapy and the Placebo Effect' is a model of clear thinking and unambiguous presentation.

In each main section of the book an attempt is made to cover the range of techniques available. Thus, in the section devoted to 'Psychopathology and Early Experience', there are clinical papers on 'Autism' by Eisenberg and Kanner, epidemiological studies relating to the hospitalization of children and to flexibility of maternal care by Prugh *et al.* and by Klatskin *et al.*, a discussion of the concept of ego disturbances in the light of psychoanalytic theory and practice by Fritz Redl, an experimental approach to the theory of introjection by Engel *et al.*, and, finally, a series of

papers on learning in animals by Hebb and by others.

In some sections the coverage is less complete. For example, there is a conspicuous lack of any genetical studies in the section on 'Somatic Factors in Psychopathology'. On the other hand, the choice of paper to represent a topic is sometimes very apt; for example, the choice of Guntrip's 'Study of Fairbairn's Theory of Schizoid Reactions', as an introduction to some of the recent developments in psychoanalysis in England, is particularly fortunate. The Index, however, is disappointing. As this book is designed to show the student the object of his study actively at work, it affords a chance for him to meet concepts that are difficult to grasp in a textbook in active use on a job of work. Such a book, therefore, is one that should have a particularly full Index so that terms can be quickly dug out from all their available contexts and scrutinized for their meanings. For example, mention in Sanford's paper of good and bad conceptions of people in the environment has no reference in the Index, despite an entry there of 'good object' in connexion with a passage in Guntrip's paper. If this book is intended for students, then so is the Index which should include copious cross-references to cognate words.

In any anthology one is inevitably scandalized by the omission of one's favourite pieces, but at the same time it is uncommon not to make some rewarding new acquaintances. In this way I read several good papers for the first time and was induced to give others a little more than that avaricious glance that is supposed to pass as keeping up with the current literature. Thus I was fascinated to read the 'Study of an Infant with a Gastric Fistula', by Engel Reichsman and Segel, and Sanford's observations during his active

participation in a 'Community under Pressure'. These reflexions only go to show how profitable it is to be under the tuition of the editors of this large and rather expensive book.

R. H. GOSLING

Autonomic Imbalance and the Hypothalamus. Implications for Physiology, Medicine, Psychology, and Neuropsychiatry. By ERNST GELLHORN. (Pp. 300. 68s.) Minneapolis: University of Minnesota Press; London: Oxford University Press. 1957.

The author, who is a leading authority in the neurophysiology of the autonomic nervous system, presents in this book the results of careful experimental work concerning the physiology of emotions with special consideration to the function of the hypothalamus. He largely confirmed Funkenstein's observations that the degree of hypotensive action of certain drugs showed greater variations in psychiatric patients than in controls. Gellhorn found that among psychopaths sympathetic hyperactivity was significantly more frequent than among normal controls while schizophrenics tended to be hyporeactors. Like other workers, he has come to the conclusion that hypothalamo-cortical discharge plays a decisive role in behaviour. He expects that before long the purely empirical physical therapies in use at present will be replaced by more rational treatments which by restoring autonomic balance will have a beneficial effect on mental illness. This book is a valuable contribution to neurophysiology and may open a new approach to the treatment of certain mental disorders and possibly also the understanding of psychosomatic disease.

E. STENGEL

GROUP AND ADMINISTRATIVE THERAPY IN GREAT BRITAIN*

The Red Hill School, Maidstone

The Red Hill School, Maidstone, Kent, is a boarding grammar school, recognized by the Ministry of Education, for the treatment and

education of maladjusted boys of very superior intelligence. The School is in no way connected with the Prison Commission or the Home Office.

* Volume 31, Parts 3 and 4, pp. 153-73.

SHORT BOOK NOTICES

Freud and the Twentieth Century. Edited and selected by BENJAMIN NELSON. (Pp. 311. 28s.) London: Allen and Unwin. 1958.

A large number of authors contribute to this account of Freud's work and its effects on modern life. It covers the impact of psychoanalysis on philosophy, religion, literature, art and society. The chapters include those on psychologies since 1880 by Prof. Gardner Murphy; Freud and modern philosophy by Prof. Abraham Kaplan; Freudianism and psychoanalysis: a Thomist view by Jacques Maritain; psychoanalysis and the history of art by Erich Gombrich; the changing concept of man by Prof. Gregory Zilboorg; and Marx, Kierkegaard and Freud by Frederick Hacker. The chapters are written by acknowledged experts, and bring together widely differing views. For example, Reinhold Niebuhr gives a Protestant's view, Jacques Maritain that of a Catholic and Prof. Herberg writes from a Jewish standpoint.

Theory of Psychoanalytic Technique. By KARL MENNINGER. (Pp. 206. 25s.) London: Imago. 1958.

This 'is not a manual of practice, but an examination of some of the psychodynamic principles operative in the practice'. Dr Menninger has drawn on his observations over 40 years of clinical and supervisory work, as well as on his experiences in conducting his seminar in technique at the Topeka Institute of Psycho-Analysis. Following the introduction and historical review, the author deals with the contract—the psychoanalytic treatment situation as a two-party transaction; the regression—the reaction of the party of the first part to the psychoanalytic treatment situation; transference and countertransference—the voluntary participation of the second party in the treatment process; resistance—the paradoxical tendencies exhibited by the party of the first part toward defeating the purpose of the contract; interpretation and other intervention—the voluntary participation of the second party, and the termination of the contract—the separation of the two parties.

Dr Menninger takes the view that the therapeutic effect of psychoanalysis is not its chief or only value, but it also essays to alter the structure of a patient's mind, to change his view of things and his motivations, to strengthen his sincerity; it strives not just to diminish his sufferings but to enable him to learn from them. He also stresses its value as an instrument of research.

Emotional Factors in Pulmonary Tuberculosis. By DAVID M. KISSEN. (Pp. 246. 35s.) London: Tavistock. 1958.

In this book a series of investigations into the part played by emotional factors in pulmonary tuberculosis is described. These investigations were controlled and were subjected to detailed statistical analysis. They cover onset and relapse and include a personality study. Some epidemiological features relating to war, race, migration and emigration, displaced persons and mental hospitals, are considered in the light of these findings and their consistency with the results of the investigations brought out. The author draws attention to the specific nature of the psychological findings and to the evidence in medical literature that parallel specific patterns may exist for other illnesses where there is a psychogenic component.

The relevance of these findings to practice is discussed, as well as their application to the broad field of epidemiology.

Revolution in Medicine. By BRIAN INGLIS. (Pp. 279. 21s.) London: Hutchinson. 1958.

This work, by a lay journalist, is a serious attempt to study the problem of stress disorder. Mr Inglis is convinced that there is a dangerous gap between the treatment that most sick people really need and the treatment they actually get. He believes that the majority of patients now on physical treatment are suffering from disorders which require a very different approach. In this book he describes research into the relationship of emotional stress to physical and mental illness and goes on to discuss the stress theory of disease. Finally he outlines the changes needed to enable doctors and patients

better to understand the nature of stress disorder: and he concludes with a consideration of some of the effects that acceptance of the stress theory will have on the community's attitude to health.

delineating its concerns and possibilities, and by focusing on crucial problems in theory and research.

Psychiatry in the British Army in the Second World War. By ROBERT H. AHRENFELDT. (Pp. 312. 35s.) London: Routledge and Kegan Paul. 1958.

Mental Deficiency: The Changing Outlook. Edited by ANN M. CLARKE and A. D. B. CLARKE. (Pp. 513. 45s.) London: Methuen. 1958.

This account of the work of psychiatrists in the British Army during the Second World War is based on the study of all available documents, published and unpublished, as well as on the author's first-hand experience of the clinical and administrative aspects of Army psychiatry. It deals not only with the wartime problems presented by the high incidence of mental illness, and the large numbers of mentally backward and mal-adjusted men in the Service, but also with the methods developed for the selection and efficient use of personnel and officers in the face of an acute shortage of man-power; the psychiatric aspects of discipline, morale, training and prolonged service overseas; the treatment and evacuation of psychiatric battle casualties in the forward areas, under difficult and varied conditions; the rehabilitation of disabled ex-servicemen, and the civil resettlement of repatriated prisoners of war.

A number of non-medical psychologists have collaborated to produce this book, which presents a review of recent work in Britain, America and other countries. It includes an examination of estimates of prevalence, criteria and classifications, and of medical and social provision for the mentally deficient; a detailed account of genetic and environmental factors in the determination of intelligence generally and in mental defect specifically; problems in the assessment of organic impairment and a review of researches relating to cerebral palsy. Half the book is devoted to the practical problems of psychological diagnosis and treatment, including remedial education, vocational and social rehabilitation, psychotherapy and speech therapy, the special problem of imbecile learning, and an evaluation of the prospects for children of mentally deficient parents. Temperamental factors in mental deficiency are discussed in detail and major follow-up studies of groups of defectives are reviewed.

Explorations in Social Psychiatry. Edited by ALEXANDER H. LEIGHTON, JOHN A. CLAUSEN and ROBERT N. WILSON. (Pp. 452. 40s.) London: Tavistock. 1958.

Attempted Suicide. By E. STENGEL and NANCY G. COOK. (Pp. 136. 25s.) London: Chapman and Hall. 1958.

This volume focuses on mental illness, and approaches the various phenomena comprehended within the term by way of two basic orientations. The first is towards human behaviour as a unitary process. As mental illness is conceived to have both intrapersonal and interpersonal aspects, concepts and methods drawn from psychiatry and from social science are required. The second orientation is towards the understanding of mental illness in terms of its counterpart, mental health.

The major emphasis throughout this book is on the effect of social and cultural factors upon individual health. The editors have tried to approach their topic in two ways: starting with the individual case of mental disorder and, with the group processes that furnish context for individual action. The authors have attempted to give form and direction to the field of social psychiatry by

This book is the fourth of the Maudsley monographs, and represents a new approach to the problem of attempted suicide, which has hitherto been regarded as nothing but an abortive form of suicide. Attempted suicide is studied as a meaningful and momentous event in the person's life with special consideration of its effects on the human environment. Five groups of persons who have made suicidal attempts have been carefully studied and the fate of the members of two groups has been followed up over considerable periods.

The authors feel that the study of the social effects of attempted suicide, in particular on human relations, opens up a new approach to the understanding of suicidal acts. Fuller knowledge of the social psychology of attempted suicide may, it is felt, lead to a reduction of the suicidal rate.

The Individual Psychology of Alfred Adler.

Edited by H. L. ANSBACHER and ROWENA R. ANSBACHER. (Pp. 503. 30s.) London: Allen and Unwin. 1958.

This is a systematic presentation of Adler's work in selections of his writings. These selections have been annotated by the editors in order to demonstrate the evolution of individual psychology; Adler's formulations of the masculine protest, the unity of the personality and the style of life, the striving for superiority and the inferiority complex, social interest, the origin of the neurotic disposition and the development of criteria of normality. Adler's first contributions to the field of child guidance are also included.

The volume attempts to show how Adler was a precursor of still other major developments in psychology, notably group dynamics and interpersonal relations.

Readings in Perception. Selected and edited by DAVID C. BEARDSLEE and MICHAEL WERTHEIMER. (Pp. 751. 66s.) London: Van Nostrand. 1958.

This collection of readings includes classical material and recent points of view and experiments. In fifty-two chapters (reprinted from a large number of journals and books), the contributors discuss the way in which we are aware of the world about us, the basic processes which give us this knowledge, and how stimuli result in organized, meaningful perceptions of the situations in which we find ourselves. Some of the papers appear for the first time in English.

The editors feel that the studies chosen represent the core of the experimental psychology of perception upon which so much current personality, motivation, and social psychological research build. The book presents a sequence of papers reporting empirical studies of perceptual processes from physiological and methodological foundations through the experiences created by simple stimuli to complex perceptual categorizing.

Psycho-Analysis and Contemporary Thought.

Edited by JOHN D. SUTHERLAND. (Pp. 149. 25s.) London: Hogarth Press. 1958.

As part of the celebrations of the centenary of Freud's birth, members of the British Psycho-

Analytical Society gave six public lectures on 'Psycho-Analysis and Contemporary Thought' in 1956. The papers included in this volume are: Psycho-Analysis and the Sense of Guilt by D. W. Winnicott; Psycho-Analysis and Child Care by John Bowlby; Psycho-Analysis and the Teacher by Ilse Hellman; Psycho-Analysis and Art by Marion Milner; Psycho-Analysis and Philosophy by Roger Money-Kyrle; Psycho-Analysis and the Current Economic Crisis by Elliott Jaques. Dr Sylvia Payne, President of the British Psycho-Analytical Society, contributes an introduction, and there is a postscript, entitled 'A Character Trait of Freud's' by Joan Riviere.

Children under Five. By J. W. B. DOUGLAS and J. M. BLOMFIELD. (Pp. 177. 21s.) London: Allen and Unwin. 1958.

This is the report of a unique study of the first five years of life of more than 5000 children from all parts of Great Britain and from all social classes. The children were all born during the same week, and have been investigated periodically ever since. The first five years are described here.

The first section of the book paints the children's home background and their changing circumstances during the survey years, in order to present the picture against which growth and health are subsequently discussed. Social class differences, both in growth and health, form the subject of the second and third sections, which also include special studies of accidents and of children admitted to hospitals. The discussion centres on the relative importance of environment and maternal care, in causing the considerable differences still found between the social classes. The final section covers a number of special topics, such as the use actually made of the welfare services, the extent to which young children are separated from their parents, and the important question of mothers with young children taking outside jobs. In each case the authors seek the effects on the children—to see whether they suffered if their mother failed to take them to welfare centres, or whether they showed any physical or emotional ill-effects if they were separated from their parents.

The book ends with a study of the growth, health and development of premature children. Class differences still prevailing in the welfare state are discussed, together with a number of other controversial topics.

The Brain and the Human Behaviour. Vol. 36 of the Research Publications of the Association for Research in Nervous and Mental Disease. (Pp. 564. 120s.) London: Bailière, Tindall and Cox. 1958.

A large number of eminent workers have contributed to this symposium. Although the title is a very general one, the actual material discussed was considered from the standpoint of what separate areas of the brain might contribute to behaviour. In addition, material dealing with pharmacological and biochemical subjects was included.

Prof. Lashley contributes an initial chapter on cerebral organization and behaviour, and of interest to psychologists in the remaining chapters are those on neural mechanisms in perception by Jerome S. Bruner, psychological defects produced by temporal lobe excision by Brenda Milner, psychosis-producing and psychoses-relieving drugs by Paul H. Hoch and a final chapter on the highest integrative functions in man during stress by Wolff, Chapman, Thetford, Berlin and Guthrie.

Diagnostic Performance Tests. By BORIS SEMEONOFF and ERIC TRIST. (Pp. 176. 32s.) London: Tavistock. 1958.

Following the trend away from the strict psychometric use of test material, the writers take the view that a testing session must be regarded as a social situation involving the interaction of personalities. As such it can yield insight far transcending the results of mechanical examination or even comparison of test scores.

This book aims to make available techniques developed in the Services during the last war. Five performance tests are presented, with norms, together with devices for comparing or combining the results with data from tests capable of group administration. The results of some factorial studies are also included. Throughout the book the psychometric approach takes second place to analysis of diagnostic aspects of test behaviour.

Topics in Psychiatry. Edited by T. FERGUSON RODGER, R. M. MOWBRAY and J. R. ROY. (Pp. 265. 20s.) London: Cassell. 1958.

This book is an edited account of the proceedings of a conference on psychiatry held in Glasgow in

October 1957. Emphasis was laid on those areas of psychiatry in which there had been recent significant changes in outlook and, in particular, areas in which viewpoints had begun to converge. Contributors were mainly from the west of Scotland, but they constitute an impressive list. The five sections are devoted to schizophrenia, psychosomatic medicine, tranquillizing drugs, mental deficiency and biochemistry and genetics.

Body Image and Personality. By SEYMOUR FISHER and SIDNEY E. CLEVELAND. (Pp. 420. 64s.) London: Van Nostrand.

The authors are concerned in this volume with elaborating a point of view that emphasizes the importance of the individual's concept of his body—his body image—in influencing his behaviour. Previous thought and research concerning body image phenomena are reviewed. New theoretical formulations concerning body image are described. In order to map out the areas in which body image concepts show exploratory or predictive value, the authors have developed a programme of research concerning the relationship of body image to such diverse variables as need for achievement, site of psychosomatic symptomatology, response to psychotherapy, behaviour in groups, cultural patterns and modes of physiological response. Finally, the results are integrated into a generalized theory concerning the part that body image plays in personality functioning.

Dreams in Folklore. By SIGMUND FREUD and D. E. OPPENHEIM. (Pp. 101. \$3.) New York: International Universities Press. 1958.

This is a newly discovered manuscript, originally written in 1911. It has not been previously published in any form. The book includes the original German text and a complete English translation. The present volume is of special significance because Freud deals here with a theme not mentioned in any of his other writings on the dream, namely dream action—a dream ending in the dreamer carrying out an action in reality. Also included is a full reproduction of Freud's letter to Prof. Oppenheim, a classical scholar, concerning their collaboration on this work.

Technic and Practice of Psychoanalysis. By LEON J. SAUL. (Pp. 244. 64s.) Philadelphia: Lippincott; London: Pitman. 1958.

This work, by a leading psychoanalyst, is an attempt to present modern psychoanalytic method as the natural outgrowth of basic theory, and not as something which is the outcome of a set of rigid rules. The subjects discussed include, goals and attitudes of the analyst, interview technique, free association, unconscious material and dreams, the conduct of the analysis, special problems, sample difficulties and abuses, and progress in and termination of the analysis.

The Objective Psyche. By MICHAEL FORDHAM. (Pp. 214. 25s.) London: Routledge and Kegan Paul. 1958.

This volume begins with an analysis of the development and scientific status of Jung's researches. Dr Fordham finds that whereas some scientists have tended to estimate analytical psychology in terms of mysticism or philosophy, theologians have recently made attempts to restrict its importance by over-emphasizing its scientific status. He contends that these conceptions arise from a mistaken interpretation of Jung's studies which, in his view, show that the unconscious processes lie at the roots of both religion and science. Following this essay are others which apply Jung's thesis to a number of different disciplines: to psychotherapy, anthropology, social psychiatry, Christian mysticism, particularly that of St John of the Cross, and contemporary events.

Young Children in Hospital. By JAMES ROBERTSON. (Pp. 103. 4s. 6d.) London: Tavistock. 1958.

This book is a complementary publication to the film 'Going to Hospital with Mother'. It is based on a memorandum submitted to the Platt Committee on the Welfare of Children in Hospital, and is published with the Committee's warm recommendation. The author attempts a systematic presentation of the problem and of some implications for hospital practice.

Mr Robertson seeks to show that the need not to separate the small child from his mother is a fundamental one. He argues that, since hospitalization puts the emotional well-being of the child at risk, it is essential that a principle of mental

health be accepted as having equal validity and applicability with the principle of asepsis already systematically invoked to safeguard his physical health. After outlining the non-separation principle and the evidence for its validity, the author suggests and illustrates some of the implications for the 'non-medical' aspects of hospital practice.

Nature, Man and Woman. By ALAN W. WATTS. (Pp. 192. 18s.) London: Thames and Hudson. 1958.

This work is subtitled 'A new approach to sexual experience'. The author has as his special concern the interpretation of Eastern thought to the West, and particularly of the type of Far-Eastern Buddhism known as Zen. In the present work he maintains that no man is fit to control nature unless he feels himself to be a part of it. Otherwise he will attack nature piecemeal. This hostility to nature is characteristic of our culture, and Mr Watts believes it to be the root of our personal anxiety and loneliness, our fear of feeling, and our reluctance to love. He goes on to discuss the origins of this alienation from nature in Christianity and Western thought, contrasting it with the Chinese philosophy of the Tao. He then illustrates the theme of man and nature with the parallel problem of man's relation to woman.

Psychoanalysis and the Social Sciences. Edited by WARNER MUENSTERBERGER and SIDNEY AXELRAD. (Pp. 297. \$6.00.) New York: International Universities Press. 1958.

The articles in this latest volume cover a wide range of topics. There are applications to the study of history and religion; the way in which individual proclivities affect the development of scientific thought, and an investigation of the psychological mechanisms underlying the effectiveness of pictorial advertising. Several papers attempt to distinguish normal adaptive functioning and mechanisms from neurotic defences—studies which have implications for a general theory of communication.

A Skeptical Psychoanalyst. By KENNETH MARK COLBY. (Pp. 175. \$3.75.) New York: The Ronald Press. 1958.

This collection of essays by Dr Colby contains his views on a wide variety of problems ranging from professional relations to research and pure theory.

He considers himself a sceptic in the ancient Greek sense of the word—one who enquires, examines, and reflects before accepting. In spite of his provocative and entertaining approach, his underlying purpose is a serious attempt to initiate fresh discussion of important questions that are apt to be slurred over under the pressure of day-to-day practical considerations.

Group Processes. Transactions of the Third Conference. 1956. Edited by BERTRAM SCHAFFNER. (Pp. 328. \$4.00.) New York: Josiah Macy, Jr. Foundation. 1957.

This volume describes the transactions of the third multi-professional conference on Group Processes sponsored by the Josiah Macy Foundation. The four main sections deal with interpersonal influences within the family, interpersonal persuasion, further studies on maternal-neonate interrelationships and Chinese Communist thought reform. The participants were Gregory Bateson, Joost A. M. Meerloo, Jerome D. Frank, Robert J. Lifton, Fritz Redl, George W. Boguslavsky, Eckhard H. Hess, John P. Spiegel, Erving Goffman, Bertram Schaffner, Helen Blauvelt, Donald H. Barron, Margaret Mead, Frieda Fromm-Reichmann, Howard S. Liddell, Frank Fremont-Smith, Alex Bavelas, Ray L. Birdwhistell, and Harris B. Peck.

Perversions: Psychodynamics and Therapy. Edited by SANDOR LORAND. (Pp. 307. \$5.00.) New York: Random House. 1956.

Seventeen distinguished contributors deal with various aspects of perverse sexual practices. The volume offers a summary of current views on perversion, and represents a unity in that all the writers accept Freud's formulations on perversions as valid. The work is divided into five sections: general problems, homosexuality, sado-masochism, other forms of perversions, and therapy.

A New Approach to Schizophrenia. By JULIUS I. STEINFELD. (Pp. 158. 21s.) London: Hutchinson. 1958.

Dr Steinfeld takes the view that behind the analysable material of a psychotic there is an inaccessible, non-analysable nucleus to which there is no psychological access. He is convinced that for the psy-

chotic there is no 'cure' possible. He attempts to demonstrate that attention to cerebral pathology is as essential as the exact appraisal of mental symptoms. The author is a psychoanalyst, and this appraisal is set within the framework of psychoanalysis. Thirty-nine illustrative case-histories are given.

Objective Approaches to Treatment in Psychiatry. By LEO ALEXANDER. (Pp. 139. 34s.) Springfield: Charles C. Thomas; Oxford: Blackwell. 1958.

In this short work, the author starts from the point of view that the causes and nature of mental disease, as well as the causes and mechanisms of response to therapy, are unknown. He urges the setting aside of unvalidated assumptions when they are a hindrance, and suggests that we analyse each step in our therapeutic action down to its least common denominator, in order to obtain the objective view which is essential to scientific evaluation. He first examines the problems of nosology, then deals with the validation of experiences by statistics. Further chapters deal with excitation and inhibition in the psychoses, electrotherapy, drug therapy, lobotomy and psychotherapy (which is viewed as a process of extinction of conditioning).

Behaviour and Physique. By R. W. PARNELL. (Pp. 134. 28s.) London: Edward Arnold. 1958.

This book is subtitled: 'An introduction to practical and applied somatometry'. Dr Parnell explains in detail the techniques he uses in physical anthropometry, and describes a number of investigations in which these methods have been used. The use of the somatochart is explained and illustrated, and the work includes a number of photographs.

Child-Centred Group Guidance of Parents. By S. R. SLAVSON. (Pp. 333. \$5.00.) New York: International Universities Press. 1958.

Slavson has formulated, and presents here, a system of re-education of relatively normal and healthy parents. He calls this procedure 'group guidance' as opposed to 'group psychotherapy', stressing the need for this form of guidance to be completely child-centred and to avoid involvements with the

personal problems of the parents where these have no direct bearing on those of the children.

Slavson discusses the origins, criteria for, procedure, pitfalls, usefulness and applicability of child-centred group guidance of parents and its effect on the child, on the parent, on the family as a whole, and on the general mental health of its members. Illustrative case material is given. Actual records of group meetings are presented and techniques of conducting the groups are described.

Teaching: A Psychological Analysis. By C. M. FLEMING. (Pp. 291. 28s.) London: Methuen. 1958.

Dr Fleming, who is Reader in Education in the University of London, deals in this work with many of the findings of recent psychological research and their relevance to problems of teaching. The various sections deal with the teacher as a student of motivation, as a promoter of learning, as an observer of growth, as craftsman and technician, as experimenter and as administrator and therapist.

Body and Mind in Western Thought. By JOAN WYNN REEVES. (Pp. 403. 5s.) Penguin Books. 1958.

This work attempts to provide an introduction to the history of psychology and also a vantage point from which to consider the present. It takes a main theme of psychological interest (that of the relation of mind and body) and illustrates, by liberal quotation, its treatment by Western thinkers, and thereby attempts to sketch the development of psychology itself.

The quotations range from Homer and Hippocrates by way of Aristotle, Aquinas, Descartes, and Spinoza to William James and Sherrington.

Psychopathic Personalities. By KURT SCHNEIDER. (Pp. 163. 18s.) London: Cassell. 1958.

Translated from the German, this work presents a classification of psychopaths, but also discusses the limitations of typology. Schneider proposes a classification into hyperthymic, depressive, insecure, fanatic, attention-seeking, labile, explosive, affectionless, weak-willed and asthenic psycho-

paths. The relation of psychopathic personality to psychosis is also explored.

The Nursing of Mental Defectives. By CHARLES H. HALLAS. (Pp. 196. 21s.) Bristol: John Wright. 1958.

Written by an examiner for the General Nursing Council, this book takes account of the new syllabus, and emphasizes the modern trends and changes in the nursing of mental defectives.

The Clinical Application of Projective Drawings. By EMANUEL F. HAMMER. (Pp. 663. £5 2s. 6d.) Springfield: Charles C. Thomas; Oxford: Blackwell. 1958.

The author attempts to tie together and to present in compact fashion the popular, yet until now scattered, field of projective drawings. In addition to the H-T-P Technique and Machover's Draw-A-Person Test, the work includes the Draw-A-Family procedure, Harrower's Unpleasant Concept Test, the Draw-A-Person-In-The-Rain modification of Abrams which attempts to elicit clues to the self-concept under conditions of environmental stress, Levy's Draw-An-Animal concept, Caligor's Eight-Card Drawing Test, and free doodles.

Experimental Design in Psychology and the Medical Sciences. By A. E. MAXWELL. (Pp. 147. 17s. 6d.) London: Methuen. 1958.

This book represents an attempt to remedy the defects in the presentation of statistics in many of the current introductory text-books. The author stresses the point that valid results in an experiment will not be achieved unless it is carefully designed. The work also aims to help those who are interested in the design of quick non-elaborate experiments to test specific hypotheses about individual patients.

The Measurement and Appraisal of Adult Intelligence (4th edition). By DAVID WECHSLER. (Pp. 297. 40s.) London: Baillière, Tindall and Cox. 1958.

This is the fourth edition of Wechsler's well-known manual. Previous editions have been extensively rewritten, and five new chapters have been added.

Certain parts have been dropped entirely. The specific changes which have been made in the revised Adult Intelligence Scale (WAIS) are discussed. A comparative analysis of the 1939 and 1955 standardizations is included.

Physiological Bases of Psychiatry. Compiled and edited by W. HORSLEY GANTT. (Pp. 344. 80s.) Springfield: Charles C. Thomas; Oxford: Blackwell. 1958.

This volume marks the Twenty-fifth Anniversary of the founding of the Pavlovian Laboratory, and is the first of the Proceedings of the Pavlovian Society. It includes a brief history of the contributions of Pavlov, and the introductory remarks also contain a summary of the concepts of Dr Horsley Gantt. The papers deal with various aspects of conditioning, and some of these are related to clinical problems.

Teach Them to Live. By FRANCES BANKS. (Pp. 287. 30s.) London: Max Parrish. 1958.

This study of education in English prisons is the product of Miss Banks's experience in South Africa and as Tutor Organizer at Maidstone Prison, where she was for nearly four years the only woman employed full-time on the personnel staff of this prison for men.

Following an introduction by the Rt. Hon. Lord Birkett, the various parts deal with the historical growth of education in prisons, students and conditions, education as therapy, education for culture and recreation, education for vocation, the means of study, the education of women prisoners, and staffing for education.

Greek Culture and the Ego. By ADRIAN STOKES. (Pp. 101. 15s.) London: Tavistock. 1958.

Applying psychoanalytic concepts to culture, the author puts forward the hypothesis of an 'ego-figure'. It expresses a balance of what is various, unlike those cultural images that emphasize the more primitive aspects of the ego. The author attempts to give body to the formal qualities of art in general, and in particular, to a side of Hellenic civilization from which he derives the birth

of science. He has tried to fill out themes in this book that were only indicated in his study of Michelangelo.

Aggression. By JOHN PAUL SCOTT. (Pp. 148. 28s.) Chicago: University Press; Cambridge University Press. 1958.

This book is intended to provide a picture of the problems of aggression, in its psychological, physiological, genetic, social and ecological aspects. It also aims at outlining some of the possible solutions to problems aroused by aggression, by reference to the newer discoveries in the basic biology and psychology of behaviour. Dr Scott takes the view that there is no evidence of any internal need or spontaneous driving force for fighting; that all stimulation for aggression eventually comes from forces present in the external environment. This leads to the conclusion that aggression between individuals and small groups can be controlled by creating environments which do not stimulate aggression.

Social Psychiatry in Action: A Therapeutic Community. By HARRY A. WILMER. (Pp. 373. 66s.) Springfield: Charles C. Thomas; Oxford: Blackwell. 1958.

The author describes the management and behaviour of a changing group of acutely disturbed psychiatric patients on the admission ward of a Naval Psychiatric Treatment Centre. By the use of certain techniques of social psychiatry, particularly in terms of daily community meetings and staff meetings, a new method for management and therapy is put forward. The author's purpose is to report an experiment, to define, to classify, to describe and to isolate certain significant elements of the structure and process, and to subject them to various methods of evaluation. The techniques used are related to those described by Maxwell Jones, T. F. Main and T. P. Rees.

Psychiatry in the Modern World. By E. B. STRAUSS. (Pp. 71. 8s. 6d.) London: Michael Joseph. 1958.

Dr Strauss attempts here to provide a guide to thinking about psychiatry as a rational discourse and indicates the place that it could and should occupy in the contemporary scene. The author also

attempts to scale the psychiatrist down to life-size, instead of leaving him in the mind of the public as a mythological construct of non-human proportions.

The work consists of five articles on psychological medicine which were published in *The Sunday Times*. It aims to satisfy legitimate curiosity about disorders of the mind that most intelligent persons seem to have nowadays, and at the same time to blunt the appetite of those who would like their psychiatry served up with a *sauce piquante*.

Extrasensory Perception and Personality Patterns. By GERTRUDE RAFFEL SCHMEIDLER and R. A. McCONNELL. (Pp. 136. 25s.) Yale: University Press; Oxford University Press. 1958.

This book represents an important contribution to psychical research. Dr Schmeidler describes some of her experiments with students, and demonstrates that a weak ESP capacity is much more widely distributed than had been generally supposed by psychical researchers. Its detection, however, in group experiments on unselected subjects, is shown to require a combination of the skills of the psychologist and the statistician. A definite relation between ESP ability and personality is demonstrated.

The Journal of the Association for Psychiatric Treatment of Offenders. New York: The Association for Psychiatric Treatment of Offenders. Vols. 1 and 2, 1957 and 1958.

This *Journal* is published by an association (APTO), a non-profit organization founded in 1950 to stimulate interest in psychiatric treatment of offenders, to conduct research into the most effective methods of psychotherapy and to pioneer the development of a community plan which would provide specially trained psychotherapists for the use of the courts and other social agencies concerned with handling the offender. The editors are Dr Melitta Schmideberg and Jack Sokol, and the editorial board includes Dr Edward Glover. A number of distinguished contributors have written articles for the two volumes of the *Journal* which have appeared.

Ego Structure in Paranoid Schizophrenia. By LUISE J. ZUCHER. (Pp. 186. 42s.) Springfield: Charles C. Thomas; Oxford: Blackwell. 1958.

Using a new technique of scoring and interpreting the Rorschach, Mosaic and Figure-Drawing tests, the author attempts to demonstrate how fluidity of ego boundaries can be evaluated in an unequivocal manner. He believes that evidence of indications of ego weakness should clarify issues involved in the concept of ego structure and lead to increased insight into, and understanding of, the essence of a schizophrenic process.

The main part of this book deals with the characteristics and peculiarities of the test performance of the ambulatory schizophrenic patient as differentiated from that of the hospitalized patient.

Anxiety in Christian Experience. By WAYNE E. OATES. (Pp. 156. 15s.) London: Allen and Unwin. 1958.

The author believes that the doctrine of the cross bears a significant relationship to the everyday anxieties of men and women in their search for integrity and redemption. He analyses in each chapter a different shade of anxiety; economic anxiety, finitude anxiety, the anxiety of grief, the anxiety of sin, legalistic anxiety, the anxiety reactions of the morally indifferent, the anxiety of the cross, holy dread related to the anxiety of the cross and, in conclusion, anxiety and the fellowship of concern.

The Matrix of Medicine. Edited By NICOLAS MALLESON. (Pp. 234. 45s.) London: Pitman. 1958.

The theme of this book is that medicine is embedded in a social matrix. Every branch of medicine is thought to have its point of interaction with society. The book is a series of studies, primarily by ordinary clinicians rather than sociological experts, into several different borderlands between medicine and society. The function of the family doctor, maternity and child welfare, health education and the community, later adolescence, industrial medicine, the climacteric, old age, congenital mental defect and nutrition are some of the subjects discussed.

Carbon Dioxide Therapy (second edition). Edited by L. J. MEDUNA. (Pp. 541. £5 10s.) Springfield: Charles C. Thomas; Oxford: Blackwell. 1958.

This is the second edition of a work first published in 1955. Eighteen contributors discuss technique and theory, clinical investigations, modification of Meduna's techniques, physiological and psychological investigations and the statistical evaluation of the results of treatment. A comprehensive bibliography is appended.

Should the Patient Know the Truth. Edited by SAMUEL STANDARD and HELMUTH NATHAN. (Pp. 160. \$3.00.) New York: Springer Publishing Company. 1955.

This book was written by twenty-four doctors, nurses, clergymen and lawyers. It deals with the conflict in the minds of doctors, nurses and the patient's family over the question of keeping away a truth that might harm the patient—that might make him feel worse, or hinder his recovery.

World Health. By FRASER BROCKINGTON. (Pp. 405. 5s.) Penguin Books. 1958.

The writer, who is Professor of Social and Preventive Medicine at Manchester University, has taken the view that the main problems in world health are now social rather than technical. Full use of science is not possible without an understanding of much deeper issues—geography, beliefs and customs, family life, population, occupation, town living, hospitals, food, and industrialisation. These social considerations include also the problems of administration. An account of the World Health Organisation is given.

Schizophrenia. By MANFRED SAKEL. (Pp. 335. \$5.00.) New York: Philosophical Library. 1958.

The first part of this new work contains chapters on the classification, aetiology, symptomatology, psychopathology, and post-mortem findings in schizophrenia. There is a chapter on prognosis and one on paranoia. The second part of this work is devoted to a description of the discovery of insulin therapy by the author, with chapters on the neurotropic function of insulin, hunger excitement, indications for the shock and a working hypothesis

to account for the results obtained with this treatment.

Epilepsy. By MANFRED SAKEL. (Pp. 204. \$5.00.) New York: Philosophical Library. 1958.

This was Dr Sakel's final work before his death. Following his work with insulin therapy, Dr Sakel became interested in the management of epilepsy, and he left behind an uncompleted manuscript, in which he expounded his theories on the mechanism and theory of epilepsy.

A Comprehensive Dictionary of Psychological and Psychoanalytical Terms. By HORACE B. ENGLISH and AVA CHAMPNEY ENGLISH. (Pp. 594. 60s.) Longmans. 1958.

This volume contains the definitions of over 13,000 psychological terms. It is intended to be a guide to the usage of these terms for readers and writers in the fields of psychology, psychoanalysis, psychiatry, education, guidance and social work. This work has a special value in view of the shift in meaning of older terms and the extensive specialization which now exists within the fields of psychology.

Problems in Intercultural Health Programs. By GEORGE M. FOSTER. (Pp. 49. 50 cents.) New York: Social Science Research Council. 1958.

This short pamphlet deals with problems involved in the formal, planned interchange of medical and associated ideas and practices across cultural and subcultural boundaries, and the sets of interpersonal relationships of many types between individuals and groups of individuals belonging to different cultures and subcultures.

The Facts of Mental Health and Illness. By K. R. STALLWORTHY. (Pp. 218. 22s. 6d.) Christchurch, New Zealand: N. M. PERYER. 1958.

This is the second edition of a book first published two years before. It is based on Dr Stallworthy's lectures on Mental Hygiene to Adult Education Classes. In it he attempts to give a simple account of psychology and of psychological disorder.

The Last Days of My Life. By JANE SIMPSON. (Pp. 168. 15s.) London: Allen and Unwin. 1958.

Miss Simpson writes of the way in which she was confined in a mental home without justification at the age of thirteen.

New Concepts of Healing. By A. GRAHAM IKIN. (Pp. 186. 12s. 6d.) London: Hodder and Stoughton. 1955.

Miss Ikin reviews some of the phenomena and problems connected with spiritual healing. There are chapters on healing—medical, psychical, psychological and spiritual; the New Testament and healing; psychotherapy and the Church's ministry of healing; psychotherapy and spiritual direction; co-operation between the medical profession and the Churches; some healers to-day; spiritual healing and the new physics, and suffering and service.

Dreams and the Uses of Regression. By BERTRAM D. LEWIN. (Pp. 64. \$2.00.) New York: International Universities Press. 1958.

This small but well-produced volume contains the seventh Freud Anniversary Lecture presented to The New York Psychoanalytic Institute in 1957.

Biographical Directory of Fellows and Members of the American Psychiatric Association. (Pp. 488. \$25.00.) New York: R. R. Bowker. 1958.

This large volume will meet a long-felt need not only in the United States but in this country as well. It lists alphabetically all members of the American Psychiatric Association with details of their training, hospital appointments and experience, publications, and present address. It will prove invaluable to those readers of American journals, who are interested in the background of the authors and their present location.

A Handbook of Medical Hypnosis (second edition). By GORDON AMBROSE and GEORGE NEWBOLD. (Pp. 276. 27s. 6d.) London: Baillière, Tindall and Cox. 1958.

This is the second edition of a book first published in 1956. New material is included, such as the use of the electro-encephalogram in hypnosis. The

section devoted to the physiology of the hypnotic state has been expanded. The section on the neuroses has been revised.

Clinical Biochemical Methods. By A. TÁRNOKY. (Pp. 239. 50s.) London: Hilger and Watts. 1958.

A description of bench biochemical tests which are accurate enough for clinical purposes, do not require highly specialized apparatus, and give results in a reasonable time.

Personality and Motivation Structure and Measurement. By RAYMOND B. CATTELL. (Pp. 948. 55s.) London: Harrap. 1958.

Prof. Cattell surveys in this book progress in the field of factor analytic research on basic personality dimensions. He presents a theoretical system based on research findings.

Psychopathic Personalities. By HAROLD PALMER. (Pp. 179. \$4.75.) New York: Philosophical Library. 1957.

The author, who is described as a leading British psychiatrist, has written a series of essays on psychopathic personalities, schizophrenia, the depressive states, the obsessions, hysteria, the epilepsies, the tension syndromes, the paranoid states and mania.

Medical Sociology. By NORMAN G. HAWKINS. (Pp. 290. 51s.) Springfield: Charles C. Thomas; Oxford: Blackwells. 1958.

The author has included in this work a historical review of his subject, a theoretical consideration of medical sociology, and a special study of some specific areas. These include schizophrenia, tuberculosis, alcoholism, cardiovascular and rheumatic diseases, geriatrics and gerontology, the interaction of folk medicine, social structure and function, organization of medical services, impact of social and demographic changes, and practical and formal aspects of research in health. He gives attention to frequently ignored assumptions and pitfalls, and the statistical discussion is clear, with frugal use of symbols. The finds of Sherrington, Cooley, Cannon, Selye, Thomas Hebb and many others are related to a comprehensive biocultural framework.

Clinical Studies in Culture Conflict. Edited by GEORGENE SEWARD. (Pp. 598. \$7.00.) New York: Ronald Press. 1958.

The many contributors to this book attempt to show, by concrete and practical illustrations, how a comprehensive consideration of behaviour broadens the general understanding of human conflict. The work represents an attempt to integrate the psychodynamic descriptions of individual behaviour with the operation of social factors. It also attempts to understand how society and culture get into not only the person but his body as well, and how the body gets into person and culture. The view is taken that the individual's internal reality, no matter how distorted idiosyncratically by the uniquely physiological and psychological determinants of the learning experience, is nevertheless an internalization of social and cultural reality.

Symposium on Preventive and Social Psychiatry. (Pp. 529. \$2.00.) Washington, D.C.: Walter Reed Army Institute of Research. 1957.

The object of this symposium was to bring together data from a number of specialized fields of social science and clinical psychiatry in order to provide a comprehensive survey of the developments during the last few years. The contributors to this symposium are very distinguished in their own fields. The six sessions reported are as follows: communication, values, influence and group structure; ecology and epidemiology of mental illness; industrial psychology and psychiatry; the significance of leadership for the mental health of groups; social psychiatry in the community, and the development of a therapeutic milieu in the mental hospital.

Hysterie, Reflex und Instinkt. By ERNST KRETSCHMER. (Pp. 148. DM. 12.80.) Stuttgart: Georg Thieme Verlag. 1958.

This is the sixth edition of a work first published in 1947. The author lays even more emphasis now on the basic problems in human biology.

Die Beginnende Schizophrenie. By K. CONRAD. (Pp. 165. DM. 17.80.) Stuttgart: Georg Thieme Verlag. 1958.

The author attempts to salvage the valuable ideas in the psychopathology of Jaspers and the Heidel-

berg school, a psychopathology which was unfortunately rendered sterile by its ties to Wundt's psychology. He also attempts to point out directions in which further research can progress.

A New Pattern for Mental Health Services in a Children's Court. By HARRIS B. PECK, MOLLY HARROWER, MILDRED B. BECK and others. (Pp. 82. 25s.) Springfield: Charles C. Thomas; Oxford: Blackwells. 1958.

This work is based on the Court Intake Project conducted at the New York City Court of Domestic Relations. It deals in the main with an attempt to broaden socialized justice in the juvenile court, and with children's court intake procedures and problems.

Man and Automation. By L. LANDON GOODMAN. (Pp. 286. 3s. 6d.) London: Penguin Books, 1957.

This book is divided into two parts: technical aspects, and social, economic, and other aspects. In the first section the applications of automation to industry and commerce are described, and in the second there are sections on the impact of automation on labour and management, on trade unions, and on technological education.

Ageing and Human Skill. By A. T. WELFORD. (Pp. 300. 25s.) Oxford: University Press. 1958.

This book is a record of the researches made in a unit sponsored by the Nuffield Foundation at the Cambridge University Psychological Laboratory, to study ways in which human performance changes during middle and early old age. It contains accounts of a series of laboratory experiments and studies in industry which are thought to constitute the most sustained research into the psychological aspects of ageing yet achieved. The primary purpose of these researches was to obtain fundamental knowledge of the processes of ageing and to point to ways of assisting the employment and training of people in their later years. The book also contributes to the psychological literature on human skill in general.

THE COMMUNICATION OF DISTRESS BETWEEN CHILD AND PARENT*

BY THOMAS S. SZASZ†

'No man is an island', said John Donne. And before him, the men who wrote the Bible observed that 'Man does not live by bread alone.' As the body requires food for survival, so the soul—or as we would say today, the social self—requires human contact lest it perish. This contact, arising in the reciprocal needs of people, is made possible by various techniques of *communication*. These techniques are of an astounding variety and range, from simple gestures and body movements to conventional signs of abstract scientific languages.

In this essay I shall focus on a very basic—perhaps the most basic—communicative method used by man, namely the communication of distress between child and parent. This is the type of communication with which everyone of us commences in his own linguistic career. Then, as parents—or even if we have no children of our own, as adults—we re-experience the use of this language when our children, or others dependent on us, 'cry' to us in 'pain'.‡ This sort of communication then, may be considered as the skeleton, so to speak, around which the body of all other, later, communications is built. The structure and function of the linguistic body, that is of

communications in general, may be illuminated by studying this core-communication and its effects on later behaviour.

THE PAIR:

CRYING CHILD—SUCCOURING MOTHER

Empirical observations concerning the phenomenology of an infant or child in distress, crying, and his mother, attempting to comfort him, can be made so easily that it would be out of place to treat this subject in detail. Instead of describing this interaction, I shall present a schematized account of the major variations which are encountered. The source of the communication—the speaker, as it were—is the child. He cries, weeps, sobs, shrieks, agonizes or, in brief, communicates what we regard as a feeling of being in distress, being hurt, being in 'pain', or suffering. What does this message do to its recipient? The listener—particularly if he is the parent who loves the child and is both legally and morally responsible for him—will become anxious, pained, or generally upset. There is, of course, no mystery to this. It is not done by the 'sympathetic magic' of voodoo rituals (which, however, probably mirror this basic human

* This paper was read at the Cornelian Corner in Detroit, Michigan, on 11 April 1959, and was presented as the The Memorial Lecture of the Society of Medical Psychoanalysts in New York City, on 13 May 1959. Manuscript received 13 April 1959.

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‡ Some comments concerning the use of certain key concepts—such as distress, pain, anxiety, etc.—are in order here. I shall use the word *distress* to refer indiscriminately to various states of unpleasurable tension. A crying infant, for example, will be spoken of as being in distress.

His distress is often interpreted by the parents as *pain*, and they may proceed to look for its causes (e.g. open safety pin, hunger). I shall prefer to speak of *pain*, however, only in those instances in which the experiencing person conceives of his distress as a disturbance referable to his own body. Occasionally the word 'pain' (between quotation marks) will be used when speaking of unpleasant feelings which are akin to *pain*, but lack a clear bodily referent. The word *anxiety* will be used to describe distress pertaining to interpersonal dangers. In this usage I have followed the principles laid down and discussed in my book, *Pain and Pleasure* (Szasz, 1957).

interaction), but simply through communication. People affect each other—and they do so profoundly—by means of messages. In this case, it is through the exposure of one person to witnessing the suffering of another.

Once this communication has occurred, the need arises in the recipient to master the 'pain'. The locus of this 'pain' or discomfort may be placed provisionally either in the infant, or in the parent, or in both. At first it will not matter to what locus the distress is assigned, but later, as we shall see, it will make a difference.

How is this 'pain' or discomfort mitigated or mastered? First, there are the simple manoeuvres of feeding the baby, changing him, cuddling him, walking with him and so forth. If any of these succeed, and the infant stops crying and becomes comfortable, the cycle of tension-and-its-mastery comes to an end. More often than not, these cycles can be terminated satisfactorily. Let us therefore turn without further delay to those instances in which the infant's communications of distress persist over long periods and defy all parental efforts to bring relief. What happens now is that the parents' tension, anxiety and 'pain' begin to mount too. Soon they feel that they can stand it no longer. They experience a panicky feeling of being *overwhelmed by tension*. This is one of the most threatening of human experiences. It matters little whether the fear of being overwhelmed results from the tensions stimulated by a child's crying, one's own sexual excitement, rage, doubt about one's (sexual) identity or whatever. In all of these instances the danger is that the ego (or self) will lose control over its destiny and crumble. This is the crisis which may terminate in a desperate social act, in an acute psychotic breakdown, suicide or homicide. Before taking such desperate measures, however, the parents usually resort to one or another of the following manoeuvres to allay their mounting tension:

(1) If the mother has been taking care of the child, she may decide to leave her task and

enlist someone else's aid. The husband, a grandparent, a baby-sitter or someone else, may be called in to help to dilute, as it were, the concentrated toxicity of listening to a crying infant single-handedly.

(2) A physician (paediatrician) may be called to help on the assumption that the child's 'pain' is an indication of illness. This may or may not be the case, but in any event the physician—his personality, advice, treatment—may be sought as an emergency buffer which the mother can place between herself and her crying child.

(3) As things grow worse, the parent's (or parent-substitute's) need to stop the baby's crying becomes increasingly desperate. This may lead to such things as letting the baby alone to 'cry himself out' in a distant part of the house, giving him sedatives or narcotics, attacking him physically, or finally even killing him.

(4) Last but not least, parents and others learn to *endure* and *bear* the child's crying and 'pain' without recourse to overt action. They learn to wait.

We are now ready to examine some of the factors which determine which of these solutions is adopted in any one case.

THE PARENTS' DREAD OF THE CHILD'S CRYING AND UNHAPPINESS

In the light of the interaction between parent and child which has been sketched, it is readily understandable that parents are disposed to dread their child's crying. Fundamentally this is no different from the general fear which people have of experiencing injuries and pains. It is the same fear once removed. However, in addition to the directly painful character of this experience for the parent as well as the child, the child's crying threatens the parent from yet another angle. The source of this threat arises from the fact that in all close human relationships which are characterized by a high degree of *mutual interdependence*, the suffering and unhappiness of one member assumes a *signal-function*

for his partner. This means that his suffering will signify not only that he is hurt or sick (which may or may not be the case) but *also* that his partner is *bad*, for he has failed to gratify his needs! Thus arises the more general idea that in all sorts of human relations one's partner's unhappiness or discomfort signifies *the badness of the self*. Once this step has been taken, its corollary follows, namely that one's *self-esteem* may be maintained or augmented by means of making one's partner happy. Making one's self-esteem dependent in these ways on the pleasure or pain (comfort or discomfort) of one's partner leads to all sorts of complications in human affairs. I shall only suggest what some of these are.

To the child's insistent crying, which sounds as if he were saying 'I'm so unhappy...' the parents' reply is, 'Please, be happy...'

We know that parents will go to great lengths in their efforts to silence a child's painful crying. Less well recognized are their efforts—sometimes going to absurd lengths—to *prevent* such crying. So-called permissive parental behaviour, it has seemed to me, often may be explained along these lines. In other words, parents may be permissive—meaning that they fail to set limits to the child's activity and permit encroachment on their own autonomy—mainly in an effort to avoid punishing the child (except perhaps by so-called discussion). Limiting a child will make him sometimes unhappy, of course, even if only for a brief period of time, and this some parents almost phobically avoid. They seem to aspire to the impossible ideal of raising their children *without ever openly hurting them*. They must always be 'good'—their goodness being defined by the avoidance of a clear, point-to-point correspondence between any deliberate act on their part and their child's crying or unhappiness. Once launched on this course, the family is headed from one trouble to another. One frequent outcome is a severe inhibition of self-expression on the part of the child.

This same pattern of the adult's inhibited self-assertion, lest it cause 'pain' to others,

may be noted in many other situations. In some of these, it has seemed to be nothing but a new edition of the pattern which has been sketched. I have repeatedly encountered, for instance, a marked reluctance in persons occupying relatively important positions to assert their rights in situations of conflicting interests. In these persons, self-assertion was significantly limited toward those who depended on them, or who occupied a subordinate relation to them. Thus, they found themselves unable to control their junior partners, employees, secretaries and so forth, but experienced no inhibition of their assertiveness with their equals and superiors. Such persons aspire to control their subordinates by setting a good example, but find themselves unable to reprimand them for open breaches of the work-contract. Their behaviour in every way resembles that of the over-permissive parent, and indeed, this is how they treat their children, too. In one of my patients, this went so far that he permitted his co-workers to 'walk all over him'—and still he found himself unable to do anything about it. When we discussed this, his response was to sketch a detailed mental picture of the complications which would result from any other course of action. He imagined that greater self-assertion would only cause distress to others—and that this in turn would lead to their aggression—and so to even more trouble, culminating in a complete dissolution of the relationship. This he was eager to avoid. In fact, he was so eager to avoid this, that time and again he found himself forgetting to state his position when face to face with his adversary, only to remember all the things which he wanted to say as soon as he was alone. Such behaviour is often ascribed to the person's anger, of which he may not be fully aware. According to this interpretation, self-assertion is curbed because it heralds 'excessive aggression'. This may be true as far as it goes, but is a rather incomplete picture of the situation, for it leaves unanswered what is 'excessive aggression'. And this is just the point. For a person is prone to feel that his self-assertion is too aggressive in

one of two circumstances. First, if he really is very angry, and harbours a great deal of pent-up rage. Secondly, if almost any kind of self-assertion, especially when directed toward those who are weaker, will seem to him as 'too much' because of its painful effect on them. It is precisely in this signal function of the distress of others that the danger lies for oneself.

It is evident from this discussion that what may appear like masochistic behaviour to an onlooker may seem like a rational and necessary compromise to the patient. And who is to say who is 'right'? Only by repeated attempts at defining one's autonomy in ways other than this can a person ascertain what the other member of the relationship is able and willing to tolerate. In some instances such attempts will lead to a breakdown of the relationship. In others, they may lead to entirely new kinds of human interchanges. My aim is to emphasize that bearing one's own 'pain' patterned after the early parent-child interaction, may often seem like the easiest—or, as it were, cheapest—way out of the dilemmas with which conflicting human needs and aspirations so often present us. Mothers, especially when harried by their children, will often express this by stating that it is easier to say 'yes' than it is to say 'no'. That this should be so is readily apparent, for it simply means that *our distress* is at least under *our own control*, whereas someone else's is not. Hence, suffering in others exposes us to the threat of uncontrolled, unmasterable tension. The suffering which we deliberately assume—particularly when it is borrowed and does not arise spontaneously from within our own body—while unpleasant enough, is at least secure in its predictability. It holds no surprises. And for the security of 'no surprises',

man is often willing to trade his freedom and much of his autonomy.*

THE CHILD'S DREAD OF HIS PARENTS' SUFFERING AND UNHAPPINESS

The parent's fear of his child's unhappiness—signalling as this might his own badness—has its counterpart in the child's fear of his parents' suffering and personal disorganization. This reaction may begin as early as the fourth or fifth year, and may become intense by the early teens. The psychological core of this attitude lies in the same signal function of the unhappiness of one's partner as has been described. The child, even more than the adult, is prone to think that if mother is upset he must have caused it. He is ready to assume responsibility and blame for the other's distress.

In addition to the basically egocentric logic of the child, however, the fear of his parent's suffering has other bases. Foremost among these is the bio-social fact that the child is dependent on the parent's integrated behaviour for his own survival and comfort. Hence, he has a pragmatic motive for keeping mother and father in good shape, so to speak. Indeed, this need to keep the parents healthy and happy is similar to the adult's need to keep his body healthy. In both cases, the selves depend on the proper functioning of other systems; in the first instance, on the parent, in the second, on the body.

Thus far we have discussed relatively general factors—that is, factors which exist irrespective of the specific family constellation—which promote development in the child of the disposition to dread his parent's distress. It is evident, however, that certain kinds of parental behaviour facilitate, while others retard, the development and flowering of this disposition.

* Leites and Wolfenstein have commented on the obligatoriness of pleasure characteristic of many adult activities in present-day American culture. They noted the connexions between the older, puritan 'goodness morality' and the new ethic of 'fun morality', by laying bare the common ground which they shared, both being techniques

of impulse control. Their socio-psychological analysis of this subject (Leites & Wolfenstein, 1950) and my attempts (Szasz, 1957, 1959) to identify and describe the communicative aspects of discomfort and comfort, may be regarded as studies of a single problem from two complementary frames of reference.

Masochistic parental behaviour is the stimulus *par excellence* to develop the child's dread of parental anguish. Parental masochism is analogous in this regard, to, say, infantile colic or chronic whininess. Both the persistently whining child and the masochistic parent say in effect: 'I am so unhappy. You must do something to make me happy!'

Striking examples of this attitude may be noted in persons, who, as children, experienced clear-cut evidences of a parent's (usually the mother's) vulnerability and weakness. These children learn through experience that it is very dangerous to overload, as it were, this beast of burden—the mother—for if overloaded, she will break down, and they will be carried down to ruination with her. The thing to do, therefore, is to 'be good to mother'. The implication, of course, is that mother is not only weak in her proneness to break down, but also that she does not know how to regulate her own burdens. This task, then, falls to the children. Self-inhibition for them becomes tantamount to security. The following clinical excerpts illustrate the salient features of this process.

This young man was raised in a home in which his mother's inadequacy in the face of various tasks was a constant irritation to his father. It seemed, in retrospect, that his mother was partly childish and weak, but partly quite capable. Some of her behaviour which was thought of as disability appeared to constitute, however, rather effective methods of sabotaging endeavours of which she disapproved. Her revolt against her husband went so far as to include a suicidal attempt, to which he responded with appropriate alarm. The unwritten motto of the family henceforth became: 'Let's keep mother happy.' This did not mean that mother was especially tyrannical, but she did keep her husband—and certainly her child (the patient)—in a state of perpetual alarm concerning her vast potentialities for unhappiness. This child grew up to become an extremely responsible individual. This manifested itself in scrupulous responsibility not only for his own actions, but also in a readiness to assume responsibility properly belonging to others. He was extremely timid in his relations to his mother,

fearing even now—when his mother showed no evidence of any personal troubles and to the contrary was conducting her life quite well—that he might hurt her by some trivial act. He displayed a similar attitude toward his superiors. While painful to him, this attitude made him well respected and contributed to his social success.

Another patient was cared for by an over-attentive, domineering mother, and a depreciated but steady father. While his mother was strong most of the time, she occasionally indulged in what seemed—in retrospect, again—like some sort of temper-tantrum. The patient remembered several of these, since they served to focus her fears on what might happen to mother (if she misbehaved, or otherwise brought on such a catastrophe). In one of these attacks, the mother sent her daughter out of the house, pulled down all the shades and lay screaming on the floor in the darkened room. She seemed to recover from this episode without difficulty. (But the patient did not.) In view of her socio-cultural milieu, this behaviour may have been neither unusual nor alarming. To her daughter, however, it meant that 'mother was going crazy'. She grew up with this dread and harboured it without modification as an adult. She entertained similar fears concerning her husband.

These clinical excerpts illustrate some of the situations which foster the conviction that self-inhibition and bearing one's own distress may be the lesser of two evils. For the child, it is a greater danger to feel that, because of lack of proper self-control, mother has become ill, went crazy or died. Then he would have to bear the loss of his most important human object and, in addition, suffer guilt for having committed a horrible crime against her.

SOME SPECIAL INSTANCES OF THE INABILITY TO BEAR THE DISTRESS OF ANOTHER

Let us apply some of the observations and ideas which have been presented to concrete situations. I shall comment briefly on three special problems, the first in medicine, the second in psychiatry and the third in the raising of children. In each, the issue of bearing the distress and suffering of another—

and the inability to do so—is crucial for the proper understanding of the communications which are being exchanged.

(1) *The physician's inability to be inactive*

The physician who assumes the care of a chronically ill and actively suffering patient is in a position much like that of a mother who cares for a colicky, whiny, sleepless baby. Both are confronted by a person whose pain they want to relieve or at least alleviate. Both remain persistently unsuccessful in their attempts to achieve this goal.

The physician caught in this dilemma, much like the patient himself, has a choice between bearing the discomfort and 'sitting it out', so to speak, on the one hand, or taking some action, on the other hand. Unfortunately, for both him and his patient, his social contract with the sick is so codified that he tends to be rewarded for unnecessary or even harmful interventions, whereas he receives nothing—or may even be penalized—for judicious waiting. I shall not belabour this point since it is familiar enough to most of us. I only wish to stress an interesting comparison between medicine and jurisprudence which highlights this problem. We are justly proud of our Anglo-Saxon laws according to which it is better to let a thousand guilty men go free than to convict a single innocent one. In medicine we seem to espouse rules which are exactly the opposite. According to these rules—which are largely unverbilized and unwritten—it is better for a physician to employ complicated, dangerous and destructive treatments (or diagnostic measures) a thousand times and fail, than it is to miss using an effective treatment (or test) in a single case in which it would have helped.

These considerations raise questions of medical ethics beyond the scope of this essay. It is clear, however, that medicine—like law, engineering or international relations—must be played according to *some rules*. It behoves us to make these rules explicit. If we fail to do this, we lose the chance to change the rules in those circumstances in which they are un-

serviceable. I submit that while most of the rules of the 'medical game' fit the needs of acute treatment problems relatively well, they do not adequately serve the requirements of problems of chronic suffering (Szasz, 1958).

(2) *The challenged psychiatrist and his response: Suicidal threats and commitment*

A typical psychiatric problem which lends itself to analysis along this same line is that of the patient who threatens to kill himself. Needless to say, I shall not go into all of the psychological and social complexities of this subject. Rather I shall focus on only one facet, namely the suicidal threat as a *communication*—a cry—to which the psychiatrist must respond.

The threat of suicide may be regarded as an adult version of crying. Both are communications and imply suffering, pleading, asking for help, saying as it were, 'I am so very unhappy—please do something to make me happy'; both may be aggressive and threatening, too. How such a plea is countered depends largely on the social context in which it occurs. In contemporary American society, medicine is practised in such a way that physicians—and especially psychiatrists—are considered as persons to whom those who suffer may legitimately turn and expect to be helped. In this respect they play a role similar to that of parents vis-à-vis their needy children. The parallel between the roles of healer and parent is, of course, thoroughly familiar and has been widely explored in modern psychiatric and sociologic writings. I wish to point out that in so far as the patient threatening suicide is regarded like a child in relation to his parents, it becomes the profession's (and the psychiatrist's) *responsibility* to prevent this action. If, however, the patient is in fact an adult, the question may be raised as to how the physician can be held responsible? Is the patient not self-responsible? And if we deprive him of the freedom to act in certain ways, are we not also depriving him of some of his adult autonomy and legal rights?

This difficult ethico-medical dilemma has

been resolved by taking recourse to demoting the patient threatening suicide to the rank of disenfranchised child. Accordingly, it becomes the physician's responsibility to prevent suicide whenever possible. This can be done generally in only one way, namely by committing the patient, usually against his will, to a mental hospital. This, of course, entails that the patient temporarily loses some of his civil rights. Moreover, even after his release from the hospital, having been committed may cause him severe injury occupationally and socially, and this may last for as long as he lives. The physician, on the other hand, is on completely safe ground when he insists on commitment. For popular opinion today demands that in these circumstances the physician act as an *agent of society* and that he carry out its wishes, which is to prevent *openly codified* suicide. The threat of suicide—or its alleged threat—is thus one of the standard psychiatric findings which may be employed if one wishes to commit someone.

It is implicit in these rules that the physician can be held responsible only for suicidal threats which he *interprets* as such. Obviously he cannot be held responsible for suicide when the intention has not been communicated to him. Thus suicides framed, so to speak, as accidents, are generally considered to fall outside the scope of medicine and the rules applicable to it. My point is simply that it is theoretically more accurate—and practically more useful—to regard suicidal threats as communications, rather than as signs or symptoms of an alleged mental illness. If we so regard them, however, we cannot penalize those who are most adept at understanding this 'language'. This would be the case if we were to take seriously the present rules of this 'game', according to which it is the physician's (psychiatrist's) responsibility to prevent suicidal threats from being implemented.

Returning to the analogy between the suicidal threat and the child's painful crying, we may formulate certain conclusions. In many cases of crying, judicious waiting, bearing the tension, constitutes the most rational course

of action. This amounts to *action by inaction*. Applying the same principle to the patient who threatens suicide implies that commitment, electric shock or tranquillizers need not be the only reasonable or acceptable psychiatric approaches to this problem.

It was more my intention to raise questions than to answer them, and I shall leave the discussion of the problem of suicide without developing this theme any further. My present aim has been limited to suggesting that the action-oriented attitudes of the standard medical and psychiatric responses to the suicidal patient may better serve the interests of the healers (and of the society whom they represent) than those of the sufferers.

(3) *The parent's inability to tolerate his child's crying*

I have already commented on the general problem of the parent (or parent-surrogate) faced with a crying, tortured child, whose distress he cannot readily relieve. I shall now consider a special instance, namely a parental response which may be paraphrased thus: 'I shall make you happy even if it kills you.' The parent's need for the child to be happy leads, in these circumstances, to an almost uncontrollable urge to make the child *stop acting* unhappy. For, if the child would only stop crying—so the parent thinks—it could be assumed once more that he was contented and happy. This urge to stop the telephone from ringing—that is, the child from crying—may lead to killing the infant. Here is such a case (quoted from the *Syracuse Herald-Journal*, 3 September 1958):

Baby's Death Laid to Dad. (Rochester AP)—A father who says 'I can't stand to hear babies cry' has been charged with first-degree manslaughter in the beating of his two-month-old son. Police said Philip Coax, 24, a factory worker, admitted striking his son, Gordon, but denied that the blows were the cause of death. He brought the baby to a hospital Saturday and told doctors that the boy had fallen out of his crib and strangled in the bed clothes. Coroner's physician Dr Walter A. Riley said a brain concussion caused death.

Occurrences such as this one are reported in the newspapers at frequent intervals. Besides the parents themselves, baby-sitters are sometimes implicated. In either case, the reason or explanation of the offending adult is that he could no longer stand the infant's (or child's) crying. I am aware that psychoanalysts or psychiatrists may interpret this kind of crime as due to the parent's hostility, conscious or otherwise, toward the child. While hostility may have some bearing on such parental behaviour, it can in no way *explain* it. Resentful or angry feelings could be satisfied just as well—in fact even better—by letting the child cry unrelieved, neglecting him, etc. The argument that infanticide of this type may be motivated by 'too much hate', as it were, may be countered further by its opposite, namely, that it is the result of 'too much love'. This argument could be supported by the popular equation of a loving attitude toward another with a sympathetic perception of *his* needs and feelings. From this point of view, excessive responsiveness to the baby's distress would have to be regarded as evidence of 'too much love'. The terms 'love' and 'hate', however, fail to give us the sort of conceptual assistance which we need to describe and explain this phenomenon. It seems to me that more relevant than the disembodied (and religiously-overladen) notions of 'love' and 'hate' is the matter of the adult's tolerance for bearing distress, both his own and that of another. Then, given his low tolerance for bearing tension, the question arises as to how he will seek mastery and relief of tension.*

* The role and significance of social factors—such as poverty, social isolation, lack of education, etc.—as determinants of certain patterns of tension-relief have not been considered in this essay. Yet, the social—in contrast and in addition to the psychological—dimensions of this problem are clearly of great importance. For instance, it is my impression that infanticide of the type discussed above is primarily a lower-class phenomenon; whether this is really so would need to be empirically verified.

SOME PRACTICAL CONCLUSIONS

My argument should allow us to draw some practical inferences. What are they? Let me conclude by stating them briefly.

(1) The adult's interaction with a crying child, especially if his own, presents him with a task similar to that which he himself had to master as a child. This is the task of *defining the boundaries* between himself and another person. All children must master the task of differentiating themselves from their parents, first however modestly, and later with the aim of achieving increasing autonomy and 'mature' interdependence. Parents faced with crying children—or doctors faced with suffering patients, lawyers faced with troubled clients, and so forth—all re-experience the same problem in a new form. The adult's task once again becomes that of differentiating himself from his partner in order to achieve a greater separateness.

(2) Only after having achieved this separation and definition of respective identities, is the parent (physician, lawyer, educator, etc.) in a position to consider the problem of his partner in a rational light. Only then can he ask, 'What can I do for the person in distress?', and answer it on the basis of reason *and* compassion, and not simply on the basis of shared suffering. Since serious suffering impairs rational judgement, answers emanating from sympathetically shared suffering are not apt to be of much value. Proper differentiation of the boundaries between helper and sufferer will also permit the development of that *autonomy* on the part of both which is the necessary basis for an *interdependent mutuality*. In this type of relationship, help may be offered or withheld depending on its rationally judged effects, rather than because the helper is emotionally driven to give or to withhold it.

(3) Having achieved sufficient separateness from the child (sufferer), the parent (helper) may find that he is unable to do anything that will relieve the child's 'pain'. This implies that the best that he can do is to *bear it*. But what

exactly does this mean? To begin with, it does *not* mean that the parent will have to leave the child, nor that he will be driven to shut off the crying. It means, rather, an *inner restructuring* of the situation in which parent and child find themselves. In large part, this is a cognitive restructuring, for it implies that the problem will now seem different than it appeared when immediate action was necessary. This does not mean that the change is purely an intellectual one. On the contrary, as in the case of any cognitive transformation, the change implies a profound alteration of feelings as well. Seeing the problem, and the task, in a new light should permit the parent to *wait* more comfortably than before. The passage of time, instead of being dreaded as the certain carrier of more suffering, will now be regarded as the likely harbinger of a change for the better.

(4) Finally, another conclusion is inherent in this line of thought. It is the realization, that, given a certain (considerable) measure of personal autonomy, and therefore of separateness between people, it becomes quite impossible for anyone to 'make someone (else) happy'. The only person toward whose happiness we can contribute directly is our own. To the happiness of others, as I see it, we can contribute at most indirectly. In other words, we can aspire to avoid making others positively unhappy, which is what we do whenever we actively interfere with their aspirations or particular techniques for mastering problems. We can, moreover, facilitate and encourage autonomous aspirations in all those with whom we come in contact. None of these things, however, will *make* them 'happy'. At best, they may provide the other person with the opportunities for maximal self-realization. It is apparent, then, that viewing the matter in this way makes it necessary to abandon the idea—and the hope—that we have the *power* (the ability, the 'love', etc.) to make someone else happy. Yet, *what we lose, he stands to gain*. So, truly, we can turn defeat into victory. For the ability to become happy now becomes a potentiality that the other

person may achieve through his own activities (and not by the 'charity' and 'love' of our 'gifts' to him). The adherence of parents, physicians and others to the seemingly painful role of trying to make others happy may then be understood, perhaps, as being partly due to the step sketched above; namely, to man's reluctance to acknowledge that '*love does not cure all!*' As long as one believes this—and psychoanalysis began to encourage this belief just when the power of Christian religious thought began to wane—all he needs to do is to try to perfect his ability to love. It would seem to me, however—and this is what I wish to emphasize—that the 'scientific reality', so to speak, of human relations is more complicated than the Bible would have it. Man must *actively* work out his relationships with those about him. And in this endeavour a loving attitude is no more than a mere beginning, or a prerequisite. Its possession guarantees harmonious human relationships and a 'good life' no more than does the knowledge of the English language guarantee the ability to write like Shakespeare.

SUMMARY

The thesis that man's social self may be studied best by examining his communicative behaviour is now shared by diverse professional persons interested in increasing our knowledge in the area currently known as behavioural science. Among all of man's communicative techniques, probably none is more basic—in the dual sense of being earliest and most consequential—than the helpless child's cry and the helpful parent's response to it.

This discussion has been divided into five parts. (1) We began with the crying child and the succouring mother, and noted that the former's distress creates a high level of tension in the latter. This requires various techniques for mastering 'pain'. (2) The parent often comes to dread the child's crying. This outcome is fostered by the parental interpretation that the cry is a sign of his own badness. This inference concerning one's self-esteem leads, further, to the insistent need to make the

other person happy. This is sought since the other person's happiness is now interpreted as signifying one's own goodness. In other words, if the child 'has fun', it means that the parent has fulfilled his duties and hence need not feel guilty or anxious. (3) A later edition of the same sort of human interaction is encountered in the frequent dread which children have concerning their parents' suffering and unhappiness. (4) Some special cases of the inability to bear the distress of another were presented. Three examples were offered: (a) the physician's inability to remain inactive and tolerate the unremitting suffering of the chronically ill patient; (b) the psychiatrist challenged by the threat of suicide and his response to it, namely commitment of the patient; (c) the parent's (or parent-surrogate's) inability to tolerate the child's crying, and his desperate attempt to silence the painful noise by killing the child. All three instances illustrate the overriding importance for the adult of the need to learn to tolerate the distress of another. Failures to learn this result in actions which are labelled helpful by the helper but which are usually experienced and regarded as harmful by the sufferer. (5) Finally, some

practical conclusions were offered. Men cling to the seemingly painful and self-defeating idea that if only they would try harder—that is, *if they loved enough*—they could make their loved ones 'happy'. The persistence of this belief was attributed, at least partly, to the hope it offers for mastering *within a person's self* what may well be problems of very much greater complexity, allowing mastery only on a quite different level. I submit that man cannot *make* his fellow man happy, for this is a task on which each man must labour on his own behalf. We can, of course, do much to provide children, students, patients, friends—perhaps everyone with whom we come in contact—with opportunities to acquire those things which will enable them to make good choices and wise decisions. This may range all the way from healing the physically sick to providing education, employment, legal protection, and so forth. Beyond this, the most that we can do—it seems to me—is to avoid making others unhappy, by depriving them of those opportunities for mastery and self-realization without which their only course lies in inviting gratification by exhibiting suffering.

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CONTRIBUTION TO THE UNDERSTANDING OF DISTURBANCES OF MOTHERING

BY IMRICH GLUCK AND MARGARET WRENN*

The common concept of motherhood is one of slavish devotion, selfless love and infinite patience. Whether any mother reaches such an ideal may be speculated upon by anyone who cares to remember his own childhood, but certainly the standards the child sets for his mother are very high. He does not recognize extenuating circumstances, and even her illness or death are experienced as evidence of her lack of love. Such standards for mothers remain latent within all of us, and show themselves in distress and urgency when, in the course of our work, we become involved with a disturbed mother-child relationship. All the more does a young mother feel the presence of these ideals, and if she is neurotic it is likely that her dealings with her children will be especially urgent and distressing. If she is preoccupied by the short-comings of her own parents, her anxiety over this is likely to be reflected in the mothering of her own children.

Difficulties arise from these matters for the psychiatrist dealing with a mother. The mother may localize her symptoms in the children and talk interminably about her difficulties in handling them; or the child's disturbance may be felt by the mother as her own symptoms. Preoccupied with the difficulties of handling her child, the mother may tempt the psychiatrist to change psychotherapy into a lecture on mother-guidance; preoccupied with her own symptoms she may make the psychiatrist curious and anxious about the absent child. In either case the psychiatrist's therapeutic task will be difficult.

Such difficulties are all the more pressing with mothers who are admitted as in-patients to hospital without their young children. They

have fallen far from the standard of motherhood expected of them in ordinary life, let alone the high ideal of motherhood latent in them; they have left their children, and in this the psychiatrist has colluded. In such circumstances it is hardly to be expected that treatment will be carried on with the optimum of mutual confidence or without major resistances. For these considerations and for the benefit of the children, there is much to be said for admitting them both to hospital when the mother is seeking in-patient treatment (Main, 1958).

THE THERAPEUTIC SETTING

This paper presents a technique for the treatment of disturbed mothers admitted to hospital for psychoanalytically orientated group psychotherapy, with their pre-school children aged 2-4 years. The technique was developed in 1957, following therapeutic experience with mothers in which no special attention had been paid to their children's disturbances. In this approach the primary consideration remained the therapy of the mother, but how the mother's illness affected the child was also studied. The psychiatrist (I.G.), who treated the mothers four times weekly in group sessions, enlisted a nursing sister and a psychologist (M.W.) to form a therapeutic team. The sister was in charge of the residence of the mothers and children, and the psychologist had the task of devising a setting in which she could meet and get to know and observe the children without hindrance. The team met once a week to exchange and record information. We wanted to study the effect of the mother's illness on her handling of her child, the way the child responded to her, what the child meant to the mother, what being a mother meant in the patient's concept

* The Cassel Hospital. Manuscript received 2 February 1959.

of herself, and any other interaction process which might occur.

The first intention was to observe the children as they lived in hospital without making any special arrangements or any special observational opportunities, but this approach turned out to be unrewarding. It proved to be difficult to get much factual knowledge of how the children lived, in spite of their being in hospital. The mothers were reluctant to disclose what went on between them and their children, and tended to conceal their children within the hospital community, by making them take long naps in the daytime, confining them to their bedrooms, and by taking them out for walks. At any moment of the day a given mother and child might be in any area of the hospital or outside. At best opportunities for observation were fleeting.

To seek a point of entry into the mother-child situation, the psychologist called a meeting of the mothers which the sister attended. The mothers came with their children and were invited to describe what it meant to come to hospital with a child, and what differences it made to the child's daily routine. The children were rather subdued and tended to cling to their mothers, but the mothers began a heated debate, each supporting her own methods for handling children and criticizing those of the other mothers.

The psychologist said she would like an opportunity to meet the children apart from their mothers and they agreed to her being in charge of a play session for their children during their own group-treatment sessions. Some mothers seemed to grant this as a concession to an impersonal curiosity of the staff, but there was evident anxiety that their methods of handling children would be criticized.

It appeared to us that the children, in their early days of admission to hospital, were under a strict and inconsistent disciplinary pressure from their mothers, who seemed very concerned that the children as representatives of themselves should behave well. Also many

of the other adult patients clearly objected to the nuisance of children in hospital—the noise and the untidiness. The mothers shared these feelings at times but at others denied them and defended their own children. We felt that the pressure of these tensions fell mostly on the children, and this impression was reinforced by some comments from mothers that their children were worse since coming into hospital or that the change had upset them. One even thought that her child had developed a special cry since coming into hospital.

THE THERAPEUTIC NURSERY

The children's play sessions which were to provide us with observational opportunities began. At first we had little apart from paint, freedom and children. The psychologist supervised in a non-directive way and observed the ways the children used these facilities. The children enjoyed these sessions from the beginning, and soon attracted the children of other mothers in the hospital. We obtained other material—sand, hammering toys, a few dolls and later a gramophone and a piano.

The sessions were well received by the mothers who appreciated them as important for the children. They collected the children at the end of each session, and swept and tidied the room.

It soon became apparent that the children were enacting and reproducing in this play some of the things their mothers did to them. For brevity we shall discuss two mother-child interactions, though instances could be quoted for all the children.

Mrs A. and her children

The mother was 36, with 4 children by her first marriage and 3 by her second. She had developed severe phobic symptoms 12 years ago and had been unable to leave her house alone. She had been treated off and on by various experts without success. Though tied to her house by phobias she continued to be a very efficient housewife and mother, admired by husband and friends for her ability to cope.

From the start of treatment she was prominent in the patients' community as a woman who understood how to manage children and could bring them up to be well-behaved. The two children she brought to hospital were conspicuously well-behaved on admission, but were unduly quiet and constrained. She was indulgent and frequently bought them small presents, but her discipline was severe. She managed the children with a skilful mixture of bribery and threats. She set herself up as an example to other mothers, and insisted that the better mothers in the hospital should organize and force the more slovenly ones to raise their standards. In her treatment sessions Mrs A. soon made it clear, by her behaviour and appearance, that she was less concerned about her symptoms than about the reputation she had to make for herself. As an experienced patient she soon took charge of the others in the group, coaching them in the evenings in their parts, and presenting her brood to the psychiatrist next day. But she also delighted in shocking the group about her experiences as a female Don Juan.

In her appearance she was somewhat startling. Though heavily covered with make-up she contrived to look un-feminine and resembled a male comedian's female impersonation. Her expression was always alert, her eyes darting about, recording and appreciating the slightest response to the challenging banter which she directed to the psychiatrist and to the other patients.

To the psychiatrist she always tried to remain friendly, but she betrayed her contempt for him by repeating and echoing his interpretations, as if to add authority to what she considered his feeble efforts. She also contrived to make his interpretations sound second-hand, by saying 'I was just going to say that', but for all this she was afraid of him. Occasionally she allowed herself to voice her anger with him, but there would follow a period of penitence, when she would speak with a whining voice, her face expressing abject misery.

The story which slowly unfolded in the group sessions helped to explain this be-

haviour. She had always felt that she was a disappointment to her parents because she was not a boy. One of her few happy recollections was playing cricket with her father. At the same time both her parents were an utter disappointment to her; her father did not seem interested in her, and for her mother she felt contempt. She remembered her as a slatternly, neglectful housewife and mother, who out of doors pretended to adhere to convention by dressing up her children and enjoyed the reputation of being the mother of such well-behaved and neat children. Mrs A. remembered how much she wanted to please, but she had discovered that her passive compliance evoked no admiration, unlike her brother who had never to earn the praise lavished on him. So she turned to actively tormenting and bullying her prudish mother, delighting in calling her rude and unprintable names, thus straining and embarrassing her. She usually managed to reduce her mother to tears, but overcome with guilt she would make up to her mother by being loving and undertaking to scrub the house, which normally looked like a pigsty. It even seems likely that her mother rather enjoyed the taunts of her daughter, and that this relationship was sado-masochistic; but the patient's deep conviction that she was not lovable made her test her mother's capacity for tolerating her, and she often asked her mother whether she loved her.

As she grew older she became a tomboy, and later indulged in compulsive love affairs. One of these led to her first pregnancy and marriage. After marriage these affairs continued, and she even decided to start a third child to prevent herself from going out with American soldiers. The onset of her phobia coincided with an appendicectomy, which seemed to prevent her from continuing an active promiscuous life. Later she persuaded her husband to buy a larger house, took in lodgers and became pregnant by one of them. She was divorced by her husband and married the father of this child.

As this story unfolded, her behaviour in treatment began to appear in perspective. Her

taunting of the psychiatrist and of the women in the group repeated attempts to shock her mother and test her mother's love and admiration for her. In teaching the other members of the group what to do in treatment and then presenting them to the psychiatrist she was identifying herself with her mother who dressed her children up and showed them off to the neighbours. The painting of her face demonstrated her homosexuality, covering up her wish to be an active, seductive son.

Her need for many children could now be viewed as a supply where, through reversal, she could definitely re-experience her own infantile need to be loved and controlled. She thus continued to provide many passive satisfactions for her children, which she would grant or withdraw according to her needs.

The phobia also began to be understood. She was afraid that her heart might stop beating at any moment and on the slightest exertion. She constantly tried to seek reassurance even from complete strangers by asking them 'Am I all right?' This was, however, over-determined, containing her fear of passivity and femininity as well as an aggressive, assertive, component, i.e. 'Do I still possess enough charm to lead an aggressive, seductive masculine life?' It gradually became clear how she obtained gratification from actively manipulating others. Being an inpatient indoors at the hospital she could forget her phobia until another phobic member of the group became able to relinquish her symptom. This was a real calamity to Mrs A. who could no longer curry favour on her own terms, but felt drawn into competition with a rank and file member of the group. She relinquished her symptom, of which she had been prisoner for twelve years. She was proud of her achievement, and on one occasion she began to boast about it to her eight-year-old son, who pointed out that this was 'not much of an achievement for a woman of 36'. She was not only hurt but felt attacked and reproached and became very anxious. As if blaming the hospital for her son's attitude,

she retaliated for a time by sending one or other of the two small children home.

As compensation, she told the group about her eldest daughter and the close friendship between them which she greatly valued. In contrast, she spoke of her elder son as a 'cissy', because he apparently always tried to please her by doing those chores which her daughters refused to carry out. Instead of praising him and being pleased, she said she felt only pity and contempt for him. The group for once refused to share her opinion, and kept defending and praising the boy. When she still persisted in asking about her son's motives, the psychiatrist pointed out that children often blame themselves when parents separate, and the son was now trying to atone for this. Mrs A. responded to this more with grief than with guilt. She was overcome by the realization that her children could also love her for her own sake rather than serve her as dangerous objects which have to be controlled. Though for a while she was depressed following this realization, she made attempts at reparation by inviting each of her children in turn to the hospital, identifying herself with the psychiatrist, who instead of condemning her for the break-up of her first marriage had helped her to understand her children. She became gradually less restrictive and critical. One day she made what was to prove a great discovery, namely that she could manage her children much better by 'talking to them', rather than 'talking at them'. She gradually came to treat them with a certain respect as individuals, just as she experienced such respect in the therapeutic group.

Peter and Jimmy, and play sessions

The gradual unfolding of Mrs A.'s history and the structure of her personality were matched or reflected at many points in the behaviour of her children when they were given the freedom of the play group.

At first they made no use of this freedom and seemed not to need or desire it. They were good, compliant children, and amused them-

selves quietly with a few personal toys or books which they brought with them, while they passively waited for Mummy to fetch them after her treatment session. They showed a colourless attitude towards the psychologist or nurse in the play room, for they had been trained by their mother to behave well and to be a credit to her. This extreme control was gradually relaxed in the permissive atmosphere of the play room.

The elder boy, Peter (4 years 2 months), adopted a maternal and therapeutic role with the younger, Jimmy (2 years 10 months), who had phobic anxieties which were apparently related to castration fears. Such anxieties, it transpired, were in fact common to these two children, and indeed was seen in her other sons who visited the hospital.

In the early play sessions, Peter and Jimmy presented no problem of management. They brought their own toys, sat close together very quietly, and were not greatly enjoying themselves. The eldest child seemed to have taken over his mother's role to her children, and when Jimmy cried out at loud noises or ran away in panic from dogs, Peter would protect him kindly, and explain to adults that Jimmy had these fears. Peter became, five or six months later, aggressive and bullying, showing his deep attitudes to his younger brother much more openly, and it became difficult for us to imagine Peter in his former saintly and benevolent role. The mother blamed us subsequently for transforming her good altruistic child into a selfish, uncivilized bully.

Four months after the beginning of the mother's treatment the relationship between the children changed and they became able to separate from each other in the play room and to be active independently. In the first stage of this dissolution of the earlier relationship the elder child began to show that his protectiveness and helpfulness was a way of taking over from Jimmy and of preventing him from finding satisfaction in doing things for himself. We then had a stage of tearful quarrelling, followed by the elder child forming a relationship with a child of his own age

and playing more boisterous and active games. The younger child, still solitary, tended to play alone or with children even younger than himself, and in general the direction of his play was regressive. Peter tended increasingly to control the psychologist and the other children.

Peter enjoyed the play sessions and readily invented games. He was a highly intelligent, imaginative boy, full of initiative. Both boys had an unusually extensive vocabulary, examined practical problems critically and were interested in finding their own solutions for things. When they needed help they approached adults with confidence, and this showed that in one respect at least Mrs A. had been a good mother. However, her disciplinary measures ignored the individuality of her children, resulting in much inner tension. The younger child's play involved compulsive messing and cleaning, and it was evident from his face that this was pleasureless and anxiety-driven. The elder would at times be driven to test out the limits of our permissiveness. For Peter the most interesting feature of the world was 'Who is in charge, who makes people do things and stops them doing things? Can I be the person who gives orders?'

Jimmy, after regressing to infantile play, began little messing and cleaning games of his own. He would ritually paint the piano and then wash it compulsively, asking vainly but regularly for furniture polish. (His mother is a compulsive house cleaner.) He also evolved a formal little ritual in which, with some pleasure, he emptied the waste paper basket and filled it again singing:

Throwing all the rubbish out
Putting all the rubbish back.

He then began to join in group games with other children, and both Peter and Jimmy eventually became social leaders in the children's group, the other children tending to imitate their behaviour.

Jimmy's painting, which always started with his hands, seemed to represent a form of

disguise. ('Today I am going to be a green boy', or 'Today I am going to be a blue boy.') He knew the names of most colours, and never made mistakes. He was rather obsessively discriminating and seemed to realize that knowledge gives control and security. The children sometimes annoyed their mother by playing with her make-up. When she was newly made-up they would ask for a kiss, and she said that she suspected what they really wanted was the mark of lipstick on their faces. Paint, making-up and altering the appearance of things were activities of great meaning to both the mother and the children. The first time the children came to a play session Peter was seen to take a handbag mirror from his pocket and to glance at himself furtively. When the psychologist asked him about the mirror he refused to answer and hid it in his pocket. This mirror reappeared at intervals in the possession of either child.

Jimmy's paintings on paper were an inchoate mass of colour, generally of one shade or a mud-coloured mixture, with a small space in the middle. If the psychologist stopped to look, he would fill the place in quickly, saying 'So that no one can see'.

Jimmy also painted the windows and the glass panels of the playroom door, in which game he was joined by Peter. This appeared to be done with the intention of concealment, and it emphasized to us his anxieties about looking and being seen. It resembled the painting of the mother who painted her face to look sexually attractive, but painted thick to hide the sort of person she felt herself to be.

Peter was the eldest child in the group, and invented activities which interested other children. He also had a drive to make use of others, and many younger and passive children lent themselves to this readily, but at times he would become sadistic. His attempts at domination were the commonest theme of his play. He would take charge of the playroom keys, and pretend to lock everything up, shouting 'I'm in charge here'.

He generally had an outburst of anger

when his mother came to collect him at the end of the session. This was first directed against the psychologist, and he would guardedly throw a little sand or an empty box. His mother did not seem to mind this, though she professed to be shocked. Later when his rage was directed against her, she became very uneasy. She could not face the fact that her child had anything but loving feelings for her, and she felt we had taken her good child and made him bad. She wanted now to withdraw the children from the play sessions, and asked if they were 'compulsory'. (Her choice of word was significant.) She began to take the children away early from the play sessions. She wondered whether the freedom of the play sessions was 'really good for the children'. It is likely that she did not carry out her wish to withdraw her children only because the other mothers approved of the play sessions. She compromised by leaving at home first one child, then the other. Later she managed to recognize her children's individual needs, with some success, though with an underlying ambivalence. She returned both children to the sessions and began to bring other members of her family to the hospital: a girl aged 10 and two boys, Nigel 7, and Roger 11. This was in part an improvement, but was also a competitive gesture towards the other patients, a denial of her fear of the hospital's influence on the children and an attempt to demonstrate that her children were 'all right' and unharmed by her handling.

Her husband frequently visited the hospital and occasionally attended the play sessions. His attitude was hostile and critical, and we felt he spoke for his wife. In the earliest days of his wife's treatment he had been used vicariously by Mrs A. as a spokesman for her critical remarks about the hospital and the staff.

Mrs A.'s name-calling game with her mother had its replica in the children's behaviour with the psychologist. In one of their more stormy leave-takings at the end of the session, when the mother said politely, 'Say goodbye', the boy shouted familiarly, 'Goodbye, Slobber-

assured that there was a controlling authority somewhere.

With the development of her new attitudes Mrs. A. became more patient and willing to listen. The aggressive violence in the children's play decreased, resulting in improved socialization. Her own improvement also helped her to separate herself from her children. Six months after leaving hospital she reported that there was now a more stable relationship between the children and herself. Peter was able to detach himself more easily from his mother and could accept the companionship of other children and his father. Jimmy, though imitating Peter in his activities, now could enjoy being the youngest child.

When Peter's four-year-old companion in the group left the hospital, we found it difficult to keep him interested in the play sessions, for the other children were at least a year younger. His play tended to take the form of spoiling the fun for others, and there were incidents of physical bullying. Once, in order to restrain him, he had to be picked up bodily and was carried out, kicking, hitting and attempting to bite. He bitterly resented this humiliating proof of his impotence, and plotted revenge for weeks after. His usual greeting was, 'I am going to bring my Daddy's knife from home and cut you up.' One day his younger brother, who showed no overt hostility to the psychologist, echoed this dreamily with his own personal version: 'I am going to bring a hatchet from home and break the piano up.' But Jimmy's attitude was more reparative and on the whole he treated the piano very kindly.

Mrs B. was a weird, psychotic personality who was suffering from a depressive breakdown which had begun three years previously when her child, Mary, was born, and for which she had been treated by electro-shock without success. The most significant aspects of her behaviour in hospital were her periods of 'absence', when left alone with Mary. Under the stress of demands from Mary which she could not meet, she withdrew and adopted a kneeling posture for several hours. In the mothers' therapy group she soon made her presence felt by an interest in spiritualism and the occult, which hushed and overawed the others. She was very attentive to Mary, but apart from her periods of absence, made great demands on her. She wanted her to be a dancer or skater, and on the occasion of

Peter was at times uneasy that his rage was tolerated and not restrictively controlled. One day he refused to allow the psychologist into the playroom. But he did not want to lose contact with her, and when she walked away he came out and tempted her to return. After some parley he made terms: he would let her in if she would telephone for him. She telephoned the switchboard and asked Peter whom he wished to speak to. He said, 'I want the Police', showing his great need to be

Mary's third birthday her otherwise expressionless face broke into a smile of ecstatic joy as she told the group that she had arranged skating lessons for the child.

The unconscious meaning of her behaviour became apparent in the treatment. In her absence and her kneeling posture she was trying to get in touch with her dead mother, from whom she drew strength. Her mother had had high social ambitions for herself, and failing in these, had sought them for her daughters. The patient had always felt she had disappointed her mother by not becoming a ballerina or famous actress. A disfiguring skin lesion on her leg had prevented her. Mary now represented to her a hope of regaining her deceased mother's love, by realizing her own (and her mother's) unfulfilled ambition.

As treatment progressed a new picture of Mrs B. unfolded. On one occasion, when the group was discussing the merits of physical illness *versus* mental illness, Mrs B. began to talk about her adolescence, when she had to nurse her sick mother (who suffered from Parkinson's disease), and who later had a stroke, resulting in a mild organic psychosis. When the group asked what Parkinson's disease was, the therapist was suddenly struck by Mrs B.'s appearance—her weirdness, her mask-like face and rigidity of posture. He pointed to her, saying that this was what Parkinson's disease looked like. Mrs B. did not become anxious, but relaxed, her face breaking into an amused smile. She was able to recognize her 'Parkinson's disease' as an identification with her mother, which explained many of her bizarre symptoms. With the realization that her peculiar and distressing behaviour was a family heirloom, she blossomed into a pleasing personality. Following this she was elected to represent the ward at patients' conferences.

This mother was one of the few who consulted the psychologist directly for advice about her child. She said the child did not sleep well and had become very babyish. Although she was nearly three, she could not

walk, and made her mother wheel her in a push-chair and carry her upstairs. She also insisted that the child was deaf as a result of an attack of bronchitis a few weeks after entering hospital. It is noteworthy that only three of the mothers in the group had asked for formal advice about their children, and in all three cases the mother complained of features in the child which the psychiatrist, and even some fellow-patients, could recognize in the mother herself; in this case, autistic withdrawal, and in the other two mothers, provocative aggressiveness.

Mary in play sessions

Mary was far from deaf, but appeared at times, very irritatingly, not to hear her mother at all; or she would say 'What did you say, Mummy?', thus forcing her to repeat herself. In the group session following the interview with the psychologist mentioned earlier, Mrs B. was amazed and indignant when the other women told her that she herself frequently seemed to be not only deaf but even blind, and that sometimes she acted as if other people were simply not in the room. Mary's mother habitually found her child's anger or distress so disturbing that she could not deal with the situation, and her method at home had been to isolate herself from the child, to walk out of the room and leave her, or to put Mary in a room by herself until she calmed down. The child was very alarmed to be left alone with her own uncontrollable feelings, and although over-dependent on her mother, had no confidence in her. When her mother was in a withdrawn mood, the child cared for her mother by surrounding her with dolls for consolation. Through identification Mary could care for herself at the same time.

One characteristic of Mary was very touching. She was usually the first child to become aware of another child's distress. If she heard a child crying she usually left her own game instantly, looked very concerned, embraced the child and offered some toy as consolation. She could experience her own feelings through the other child.

Mary began gradually to alter her ways of achieving security. She would project her insecurity on to the dolls in the playroom, and then on to the psychologist. She would construct a secret nursery under a table, which she surrounded by chairs, hedging herself and the dolls in safely from the outside world. As she became used to contacts with the other children, and could even enjoy participating in games with them, she gradually abandoned this secret retreat; but when she sensed any danger in the playroom, she would

Mary's mother remarked with satisfaction that whereas formerly Mary had only one

relationship in the world, with her, she now found it easy to form relationships with others. She did in fact leave her mother alone quite a lot, solving the mother's problem of finding it difficult to tolerate the child for more than a limited time. The maturation of the child's social attitudes contributed much to diminishing the tension between her and her mother.

In this way Mary coped with the problem forced on her by her mother's personality. Her mother had quite genuinely only a limited tolerance for another person in an intimate relationship, and in this respect she is not likely to alter greatly. Mary will have to make the best adjustment she can to a partially unsatisfactory mother, who is grateful for her daughter's capacity to detach from her and to relate to other objects and people more independently.

OTHER POINTS OF INTERACTION

Though of necessity we have restricted ourselves to the description of only two mothers and their children, the group as a whole consisted of 'fairly good mothers'. They appeared at first sight not to be unduly harsh, controlling or rejecting, but seemed to have genuine concern and love for their children. Their problems with their children arose from neurotic difficulties which resulted in methods of handling which in circular fashion made life difficult for both mother and child.

Regardless of their own psychopathology, some mothers tended on admission to regard their children's behaviour rather than their own problems as being in need of treatment and they saw themselves as suffering from the stress of mothering. The hospital could give them no relief from this task, but it accepted instead their difficulties, examined their relationship with their children, and helped them to recognize their own contributions towards the disturbance. Feeling that their illness had affected their children, they tended to present their children for treatment, and it was only after they were sure that their children's problems were recognized that they

began to feel that they themselves had a right to treatment. The accent shifted to their own problems, enabling them to manage their children better, and to seek help for themselves without guilt. Following this, the mothers could more readily accept the psychologist's offer of play sessions for the children.

Certain other mothers did not present their children's problems as leading symptoms. Only they were ill; their children were 'good'. The play sessions, by revealing the child's capacity for object relationships, led us to understand the 'model child', of which Mary is one example. By this we mean a child who causes no apparent anxiety to the mother but who becomes a source of concern to the psychologist during the play sessions when the mother is absent. In contrast to others, the 'model child', previously regarded as contented and tractable, reveals himself in his pitiable compulsive withdrawn play from which he derives no pleasure. His capacity to use people is narcissistic and limited to rare demands for admiration. Such a finding in the play sessions becomes more significant when viewed side by side with the psychiatrist's discovery of the mother's lack of awareness of it. The fact that she may find satisfaction in such a child can point to significant aspects of the mother's psychopathology which would otherwise be missed. These children were indicators, diagnostic aids, which helped the mothers' therapist.

In treatment the continuous working through of the mother's neurotic conflicts resulted in what we called the 'paradoxical response' of the child. Early in treatment it was possible to note increasing ease in coping with her child's difficulty, but later the mother would become distressed by the child's inability to accept her new, and certainly healthier, attitude. It seems as though the mother's previous handling establishes a dynamic situation, the alteration of which is resisted by the child. The child, in an attempt to re-establish the previous relationship, now attempts to control the mother by the very

technique by which he was controlled by her in the past.

Johnny, aged two, expressed in no uncertain terms his disapproval of his mother and the psychiatrist. He found his own way into the staff common room, and seeing the psychiatrist enjoying some degree of leisure, crept up to him and, to the amusement of the occupants, proceeded to glare at him in silent anger until satisfied that his disapproval had registered.

Mrs B. complained about her child's behaviour in one of her group-treatment sessions. She said that lately she had begun to read stories to Mary in an attempt to amuse the child, but, instead of appreciating this, Mary would interrupt in the middle of an exciting story and ask dreamily, 'What are you saying, Mummy?' The group members observed with satisfaction and shrewdness that Mrs B. did just that in her sessions. They reminded her that she too often interrupted their stories and particularly the psychiatrist's interpretations, saying that she did not hear or understand what they or he had said. Mrs B. acknowledged this attack by asking provocatively, 'Do I really?', and betraying her satisfaction by a contented smile. She could now see for the first time the satisfaction that she derived from hurting people.

The mother's apparent improvement and the consequent new disturbed mother-child relationship seems to represent a subtle form of resistance of the mother to her treatment; a resistance in which the mother's negative transference feelings seem to be expressed through the child. By disregarding the child's real needs, while apparently acting in collusion with the psychiatrist's wishes, the mother attempts to defeat the treatment. The mother experiences the child's rejection of her new behaviour as a retaliation which is particularly painful to her because it represents in caricature her own unconscious hostility to the child.

Thus a mother might gain 'insight' but because of underlying emotional reasons be unable to utilize it in her relationship with her

child. Consciously she now fulfils her role of an 'improving' patient; unconsciously however she retains her characteristic object relationship with her child, but it is now reversed; she shows, in resistance, that toleration of her child's spontaneous behaviour according to his needs is dangerous. This means that the child can still represent to the improving mother a threatening phantasy object rather than a real person. An important phase of treatment has had to be the analysis and interpretation of such phantasies, in order to gain real improvement in the mother-child relationship.

The following examples illustrate the psychopathology involved:

Mrs A. felt loved by her mother only when she assumed a masculine role towards her. This prevented her from accepting and enjoying her femininity. Her rigid control and devaluation of her boys represented her envy of masculinity and the denial of her own femininity. Any sign of initiative and spontaneous behaviour from the boys endangered this phantasy, and with it the gratification which she derived from it.

Mrs B. hoped to propitiate her dead mother by identifying with Mary who thus represented her own ego ideal. She now expected Mary to fulfil her own mother's expectations of herself. Mary became a narcissistic object tolerable only in so far as the child was able to meet these high expectations. At the same time, Mrs B. had to deny her child's own needs because the awareness of these would have revived the memory of her own disappointment in her physically and mentally ill mother. Mrs B.'s 'Parkinsonism' represented a reparative identification with the dead mother, the derivative of which we could see in Mary's capacity to feel for her mother or for any other person who is in distress. Mrs B. was able to communicate to Mary not merely the disappointing but the loving experience with her own mother. For in spite of shortcomings her mother had offered (by reason of illness and the necessity of being nursed) a mutually dependent relationship which laid

the basis for her later capacity to form an object relationship. The short-comings of such a relationship could be seen in her relations with Mary.

The value of the play session

In the playroom we tried to augment or modify the parental image. As a mother's attitude comes to be changed in treatment, she is able to form a healthier relationship with her child, and the child, aided by experiences obtained in the independent environment of the playroom, is ultimately able to respond to the changed attitudes of the mother. This does not come about by offering the child an individual adult as a substitute for its mother. In our experience the child was initially hardly aware of the adults in the playroom as people, and often failed to recognize them in other parts of the hospital. It experienced these individuals as part of the atmosphere of the playroom, and this was confirmed by the way in which one individual could be accepted for another, perhaps on the grounds of their similarity as 'not mother'. The formation of a temporary relationship with another adult for short periods during the week seemed to be a reassuring rather than a confusing experience, contributing ultimately to a more satisfactory relationship with the child's mother.

CONCLUSION

(1) The clinical insight derived from the simultaneous observation of mothers and children is greater than if restricted only to one of them, particularly in view of the close tie between pre-school children and their mothers. The clinical material can be seen as deriving from two sectors of the same living field. To admit a mother to hospital without offering an adequate setting for the child is in our experience disturbing, and unrewarding to both mother and psychiatrist.

(2) The therapeutic team should be well equipped to carry and resolve the emotional stresses imposed by work with disturbed mothers and their children. To function adequately, the members of the team must be relatively free from anxiety, and to aid this, the role of each should be clearly defined and respected.

ACKNOWLEDGEMENTS

This work would not have been possible except in a hospital which had a therapeutic and administrative climate which could permit the development of the therapeutic setting here described. Special thanks are due to Dr Main, Dr Rubinstein and Miss D. Weddell for their support, to Sister E. Gazdar and Dr Eric Rayner for their active collaboration.

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DEPRESSION: THE USE AND MEANING OF THE TERM

BY MYER MENDELSON*

The meaning of the term 'depression' is unclear and ambiguous. There is no widespread agreement about what phenomenon is actually being described, no consensus as to the internal psychic atmosphere implied by this expression and no unanimity about the relationship of depression to allied psychic states.

The purpose of this paper is to review critically the literature on depression in order to see how this phenomenon has been conceptualized by various observers and what its relation has been thought to be to various other psychopathological disorders. The diagnostic confusion centring around the term 'depression' will be discussed. The relationship that some writers have seen between manic-depressive psychosis and schizophrenia will be considered and various psychodynamic formulations of the 'depressed' patient will be examined.

DIAGNOSTIC CONSIDERATIONS

The continuous refinement of clinical theoretical models depends in large measure on the ever more precise delineation of the clinical material on which the models are based. Freud (1925) and Abraham (1925) were particularly careful to identify diagnostically, and by implication descriptively, the patients whose symptoms or productions they were discussing. In order to corroborate the diagnosis of two of his patients Abraham went so far as to testify that they had 'repeatedly been put in asylums or sanatoriums where they were under the observation of able psychiatrists and... (that) they had been examined by eminent mental specialists. The

clinical picture was absolutely typical (of manic-depressive psychosis) and the circular course of the illness quite characteristic in both cases'.

This attempt of Abraham's to identify as accurately as possible the patients from whom his theories were being derived contrasts with the frequent elaboration of theories based on case material that is described no more precisely than by the word 'depressed'. Not only are the emotional disorders not clearly identified but the size of the sample from which the theories are derived is rarely explicitly made clear.

One could be very respectful and appreciative of any particular worker's range of experience and still wish to have more precise knowledge about the sample of patients he is reporting. Kubie (1952) after twenty years of psychoanalytic practice reports that 'even in the course of a lifetime of exclusive devotion to the psychoanalytic treatment of patients, no analyst will have been able to analyze deeply more than a few representatives of any one psychopathological constellation'. Mabel Blake Cohen and her colleagues, after extensive preliminary research lasting from 1944 to 1947, intensively studied a group of manic-depressive patients and finally reported on a series no larger than twelve cases (1954).

Of course diagnostic categories are not entirely reliable. They may be misleading sometimes but nevertheless they are still the daily currency with which psychiatrists, in common with their medical colleagues, conduct their affairs. And even if this currency does not represent the pure gold of another era, neither can it be said to be entirely debased. The diagnostic categories must represent some approximation to clinical reality or else they would have gone the way of devils and

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humours in psychiatric thinking. Even psychoanalysts, who more than any other psychiatric workers tend to focus on the motivational and experiential in contrast with the formal and diagnostic aspects of psychiatric illness, implicitly and explicitly acknowledge the usefulness of the clinical categories.

*Manic-depressive psychosis and
involucional melancholia*

Most current psychoanalytic writers on the subject acknowledge with relatively little hesitation the usefulness of the diagnostic category of manic-depressive psychosis. Cohen and her colleagues (1954), for example, consider that 'the manic-depressive syndrome does represent a fairly clear-cut system of defenses which are sufficiently unique and of sufficient theoretical interest to deserve separate study'. And Jacobson (1953) too indicates quite explicitly that she regards manic-depressive psychosis as a distinct clinical entity.

Whether or not to include involucional melancholia in the manic-depressive category at one time aroused considerable controversy in the clinical psychiatric literature. After considerable vacillation this condition was finally established as an independent diagnostic entity, primarily as the result of a series of papers the most significant of which were those of Titley (1936), Palmer & Sherman (1938) and Malamud, Sands & Malamud (1941). These papers, all based on the comparative study of involucional melancholic patients with manic-depressives, and also in Titley's paper with normal subjects, can be summarized as arguing that involucional melancholia is different from manic-depressive psychosis in at least two areas, prepsychotic personality and symptomatology.

Prepsychotic manic-depressives are described by Henderson & Gillespie (1950) as having 'frank, open personalities. They are either bright, talkative, optimistic, aggressive people, who make light of the ordinary affairs of life, or else they take a gloomy outlook, bewail the past, make mountains out of molehills; or

there is a combination of the above moods, rendering the person emotionally unstable and variable.' While differing in some details, this description also characterizes the prepsychotic personalities of the manic-depressive patients whom Titley and Palmer & Sherman studied.

Contrasted with this picture is Titley's description of the prepsychotic personalities of involucional melancholics: 'Narrow range of interests, difficulty in adjusting to change, limited capacity for sociability and friendship, rigid adherence to a high ethical code, marked proclivity for saving, reserve that becomes positive reticence so far as intimate matters are concerned, an ever-present anxious tone, profound stubbornness, overwhelming conscientiousness and strained meticulousness as to person and vocation are constant concomitants of the group.' This observation was quite typical of the findings of the other writers, all of whom were quite agreed that the involucional melancholics and the manic-depressives whom they studied differed markedly in their prepsychotic personalities.

It is of interest to note that recent psychoanalytic descriptions of the prepsychotic personality of the manic-depressive correspond rather closely to the older clinical psychiatric pictures. Jacobson (1953), for example, mentions that 'they may be delightful companions or marital partners, a feature that Bleuler mentioned especially. In their sexual life they may show a full genital response, and emotionally... a touching warmth and unusual, affectionate clinging to people they like.'

This description as we have seen is markedly different from that of the prepsychotic personality of the involucional melancholic referred to above. However, no comparable psychoanalytic studies or descriptions of the prepsychotic personality of involucional depressives exist. As for the actual psychoses, Fenichel (1945) remarks that 'psychoanalytically not much is known about the structure and mechanisms of involucional melancholias. They seem to occur in personalities with an

the event that led some psychiatrists to imply that a schizophrenic who had previously been diagnosed manic-depressive had really been schizophrenic all the time or whether there actually were some pathognomonic schizophrenic signs which could guide an experienced observer to the correct diagnosis even in the early deceptive phase of some schizophrenic illnesses. They concluded that 'nearly all errors of diagnosis were made not because of insufficient observation of symptoms but because of failure to interpret the diagnostic significance of the symptoms'. They emphasized that 'even a trace of schizophrenia is schizophrenia and has a very important prognostic as well as diagnostic significance'.

This view is in agreement with that of Bleuler (1950) expressed many years before: 'The symptomatological differentiation of schizophrenia from manic-depressive psychosis can only be based on the presence of the specific schizophrenic symptoms. All the phenomena of manic-depressive psychosis may also appear in our disease; the only decisive factor is the presence or absence of schizophrenic symptoms. Therefore, neither a state of manic exaltation nor a melancholic depression, nor the alternation of both states has any significance for the diagnosis. Only after careful observation has revealed no schizophrenic features, may we conclude that we are dealing with a manic-depressive psychosis.'

In support of the above observations one might also cite the geneticist Kallman's (1953) observation, based on a most extensive series, that schizophrenic and manic-depressive psychoses do not occur in the same family unit if the diagnostic criteria for these conditions are properly restricted.

Psychogenic relationship

Attempts have been made to erect theoretical models which would clarify the psychogenic relationship of manic-depressive psychosis and schizophrenia to one another. It has been mentioned that Fenichel saw the pathogenic fixations of schizophrenia as

dating back to an earlier stage than those found in the depressive reactions. Melanie Klein (1948) and Fairbairn (1952) subscribe to the same thesis, which however they amplify in considerable detail. Klein's theory centres around the child's attempt to achieve satisfactory object relationships in the presence of conflicting feelings of love and hate. The infant is believed by Klein to enter a phase which is characterized by a complex mixture of feelings consisting chiefly of grief and sorrow over the feared loss of his love object, a state of affairs which he attributes to his own greedy hostility and because of which he feels guilty and self-reproachful. The period when the child experiences this combination of guilt and fear and sorrow and self-reproach is referred to by her as 'the depressive position'.

It is in this period, Klein believes, that the infant must come to terms with the realization that the object he hates, the 'bad object', and the object that he loves, the 'good object', are in reality one person, a 'whole object'. She feels that it is only when the mother can be identified as a whole, real and loved person—and not divided up into gratifying and frustrating part-objects or breasts—that the infant can be said to be in the depressive position. This depressive position is believed by her to be characterized by fears on the part of the child that his hatred and aggression will prove stronger than his love and will result in the lasting loss of his object. Klein believes that in a favourable outcome the child becomes confident of his mother's love for him and of his own capacity to love, i.e. he succeeds in establishing his 'good objects' securely within his ego. She postulates that a predisposition to depression arises only if the infant fails to establish his loved objects within his ego, i.e. only if he fails to develop feelings of trust and belief in his objects and in his own capacity to love. In other words she believes that those infants who fail to pass successfully through this so-called depressive position remain liable to succumb to depressive feelings again and again throughout their lives.

She feels that before he experiences the feel-

ing of being loved, i.e. before he can identify the mother as a whole object who loves him, that he is in a phase which is characterized by anxieties about his extreme rage and sadistic fantasies. She postulates that this anxiety is experienced by the infant as a feeling of helplessness and fear in the face of the immense tensions inside himself and of the external persecutors who are the projections of his hate and anger. She believes that a person who has never worked his way through what she refers to as this 'persecutory-schizoid position' and who has never experienced a satisfactory love relationship in his infancy may all his life have a markedly distorted sense of reality since his persecution anxieties cause him to see people mainly from the aspect of whether or not they are persecutors. She believes that for such an individual a satisfactory relationship with another object in the sense of seeing it and understanding it as it really is and loving it, is not possible. And it is this position, she holds, that underlies paranoid and schizophrenic disorders.

Fairbairn differs from Klein in several important theoretical points. He too considers the depressive position as underlying manic-depressive psychosis, but his view of this and of the 'schizoid position' is considerably different from Klein's. He feels that the child's oral relationship with his mother is his first experience of a social relationship and the foundation upon which he builds all future relationships. According to Fairbairn, a person who is fixated at the late oral phase, after the acquisition of teeth, remains in the depressive position in which his great problem is how to love without destroying by hate. But Fairbairn postulates that in the pre-ambivalent early oral phase, when the child relates to his mother only by sucking, it might appear to the infant who feels that he is not loved that the reason for this is that he has destroyed his mother's affection and made it disappear by the very act of his sucking, i.e. by his first way of expressing love. The intolerable situation in which he then finds himself is that he perceives his own love as destructive and bad.

The result is that he comes to regard expression of his love as bad, and comes to feel that love relationships in general are at the very best precarious. This is the 'schizoid position' as Fairbairn conceptualizes it. He believes that it underlies not only schizophrenia but the very comprehensive schizoid group.

It was in the context of this metapsychological scaffolding that Fairbairn was impressed by the difference between two sets of complaints both hitherto designated as 'depressive'. Ever since Freud's classic paper (1925) the correlation between depression and self-directed hostility or aggression had established itself as the chief formula of the depressive illness. For Fairbairn too, as we have seen, depression is intimately associated with aggression. He believes however that there is an earlier and more basic reaction than anger to the loss of the object. This he refers to not as 'depression' but as the 'schizoid' way of reacting. Patients who react in this way to object loss will frequently describe themselves as being depressed and are considered so by their therapist. Whitehorn (1939) had long ago commented on the naïveté of uncritically believing that one can easily label a patient's emotions by listening to the conventional terms which he uses to describe them. It would seem that nowhere is this danger more threatening than when the patient states he is depressed. Many patients on elaborating on what they mean when they say they are depressed will use such words as 'lonely', 'empty', 'a feeling of futility', 'hungry', etc. These 'complaints of feeling cut off, shut off, out of touch, feeling apart or strange, of things being out of focus or unreal, of not feeling well with people, interest flagging, things seeming futile and meaningless' all describe a state of mind which Fairbairn refers to as 'schizoid states'. Patients often call these feelings 'depression' but this state 'lacks the heavy, black, inner sense of brooding, of anger and guilt, which are not difficult to discover in depression'. In these schizoid states there is frequently no communication or indication of guilt or anger—just a

sad, empty loneliness that appears quite different in quality from the retarded depression of the more cyclothymic person. Guntrip (1952) comments on this type of reaction as follows: 'When you cannot get what you want from the person you need, instead of getting angry you may simply go on getting more and more hungry, and full of a sense of painful craving, and a longing to get total and complete possession of your love-object so that you cannot be left to starve.'

In the same vein Good (1946) divides depressions into two groups. The first group consists of those patients ordinarily classified as in the depressed phase of manic-depressive psychosis. The patient belonging to this group has lost touch with reality, is retarded, complains in a low monotonous voice of having committed grave and unforgivable sins, of being eternally damned and destined to endure the tortures of hell. He reproaches and vilifies himself and may refuse food or may demand that he be done away with.

A typical patient in the second group retains the ability to smile and to communicate and may appear deceptively normal. He may describe his complaints in various ways: "I don't get the same kick out of things as I used to", "I feel as if I had a big disappointment only it lasts", "things don't seem worth while," "fed-up," "browed off," "as if the joy had gone out of life", of the color having faded from life, of life being insipid, tasteless, dull, and monotonous; "things don't matter" or "I don't give a damn—nothing left to bother about—not worth carrying on for", etc. Seldom do self-reproaches accompany the verbal expression of these feelings. Good refers to this second type of depression as 'schizophrenic depression' in contrast to the first type which he designates as 'melancholic'.

By 1951 we find that Rado (1951) had lost his earlier certainty about the psychodynamics of depression. He stated that 'we encounter depressions in drug-dependent patients, neurotics, schizophrenics, general paretics, patients afflicted with severe physical

illness, etc. The question arises whether or not significant psychodynamic differences exist between depressive spells that occur in different pathogenic contexts. Further psychoanalytic investigation may provide an answer to this question.'

Indeed it was a pertinent question. Are the formulations chiefly derived from the study of a small group of manic-depressive patients as applicable to the restlessly agitated depressive psychoses of the involutional period, the adolescent schizoid states and the listless post-viral depressions as they are to the deeply retarded periodic melancholias of the manic-depressive type? It is one which workers had tended to ignore or to answer with misplaced and unwarranted confidence.

Gero (1953) addressed himself to this same question. He uttered a warning about the dangers inherent in not clearly recognizing the nature of the clinical material forming the basis of any particular theory. He pointed out that 'we do not always realize that we are talking about different phenomena and arrive at theories which contradict each other'. He not only believed that all depressed patients do not necessarily belong to the same clinical groups which Freud and Abraham studied, but he also felt that even 'in the same type of depression different aspects of the symptomatology necessitate different explanations'.

It is noteworthy that Engel & Reichsman (1956) in their study of the infant Monica conclude that their observations lead them to the position that 'in depression there is not only the active, oral, introjective anlage emphasized in classic theory' (characterized by 'the varieties of internalization of aggression') but 'also an active, pre-oral, pre-object anlage' which is 'psychologically...well expressed by Bibring's (1953) phrase, "the ego's shocking awareness of its helplessness in regard to its aspirations"'. These conclusions appear to bear striking kinship to those of Fairbairn and of Good although expressed in different terms and derived from different patient material.

Stunkard (1957) too is impressed with how little the classical psychodynamic formula-

tions of depression are applicable to the depressed obese patients whom he has been studying. He describes these patients as follows: 'The predominant symptomatology was depressive, but they were not typical depressions. The closest parallel is perhaps those curious periods of apathy and sadness which occur during the adolescence of schizoid people.' He particularly notes the absence of expressions of self-condemnation.

IMPRESSIONISTIC WORD PICTURES

Perhaps it is as a result of this insufficiently clarified differentiation between the various kinds of depressed states that different writers have so variously conceptualized the unconscious meanings of the depressed mood. Many writers have painted impressionistic word pictures of depression which somehow partake of psychoanalysis as art rather than psychoanalysis as science and may very well represent profound personal intuitions as much as they depict the raw clinical data.

For Abraham (1925), for example, the depressed state appears to have been a complicated process of psychic digestion shot through with primitive desires and impulses and fantasies. Freud (1925) saw melancholia as a loud-lamenting, self-tormenting period of mourning in which each and every tie with the introjected love object is painfully loosened and abandoned. Klein (1948) and Fairbairn (1952), as will be recalled, think of depression as a mixture of sorrow over the loss of the love object and of guilt over the hostility and rage that brought about this loss. Balint (1952), by way of contrast, thinks of depression as essentially a state of starved unhappiness not necessarily reactive to previous sadistic fantasies. When the components of hostility and guilt are absent from this complex of feelings, Fairbairn, as we have seen, refers to it not as depression but as a schizoid state.

Rado (1928) pictured a depression as a great despairing cry for love and forgiveness, a drama of expiation acted out on the psychic plane following upon a fall in self-esteem.

Fenichel (1945) too described depression as being simultaneously a fall in self-esteem and an attempt to coerce the love object into delivering the narcissistic supplies necessary to restore the self-esteem. He and Rado both felt that this formulation was broad enough to apply to all cases of depression.

Bibring (1953) and Jacobson (1953) also feel that there is a mechanism common to all cases of depression, but they differ somewhat from Rado and Fenichel in their conception of it. They see the fall in self-esteem as the essential element in depression. They believe that all else—including the restitutive attempts when they occur—are secondary phenomena.

Furthermore from each personal vision of depression stem derivative explanations of one or another depressive symptom. The guilt of which the depressive complains, for example, was viewed by Abraham (1925) in conformity with his particular picture of this condition as related to the patient's cannibalistic impulses. Rado (1928), with his conception of depression as a prolonged attempt to win back the love object, understood the patient to be guilty 'because by his aggressive attitude he has himself to blame for the loss of the object'. Cohen and her group (1954), however, think of the patient's guilt as essentially a device to manipulate the object and to win approval. As they put it, 'the patient merely resorts to the magic of uttering guilty cries to placate authority'.

CIRCUMSTANCES AND NATURE OF LOSS OF SELF-ESTEEM

To go one step further, those writers who have concerned themselves with the importance of loss of self-esteem in depression have described the circumstances and the nature of this loss in somewhat different terms. Rado (1928), for example, thought that self-esteem, in the type of patient that got depressed, was essentially dependent on external narcissistic supplies. Depressive patients 'have a sense of security and comfort only when they feel themselves loved, esteemed, supported and encouraged.... They are like those children,

who when their early narcissism is shattered, recover their self-esteem only in complete dependence on their love objects.'

On the other hand, Fenichel (1945), while agreeing that depressions occur in persons who are fixated at the point where their self-esteem is regulated by external supplies, appeared to have a broader conception of the events that cause a fall in self-esteem in depressives. In addition to the loss of love, whether by disappointment or by the death of the love partner, he included among the possible precipitating circumstances, 'experiences, which, for a normal person, would also imply loss of self-esteem, such as failures, loss of prestige, loss of money, a state of remorse... or further they may be tasks which the patient has to fulfill and which, objectively or subjectively, make him more aware of his "inferiority" and narcissistic needs; paradoxically, even experiences that for a normal person would mean an increase in self-esteem may precipitate a depression if the success frightens the patient as a threat of punishment or retaliation, or as an imposition for further tasks, thus augmenting his need for supplies'.

Bibring (1953) and Jacobson (1953) agree that loss of self-esteem is basic to all types of depression. And since they also agree that a lowered self-esteem may have many different causes, they also necessarily feel that depressed states may arise from a multitude of sources. Bibring defines depression as 'the emotional correlate of a partial or complete collapse of the self-esteem of the ego' and as 'the emotional expression... of a state of helplessness and powerlessness of the ego, irrespective of what may have caused the breakdown of the mechanisms which established (the) self-esteem'. He feels that depression results from the tension between highly important aspirations and an awareness of real or imaginary helplessness to fulfil these aspirations. He classifies these aspirations and postulates that each set of aspirations is correlated with one or another developmental level. He describes the narcissistic aspirations associated with the oral level as 'the need to

get affection, to be loved, to be taken care of, to get the "supplies", or by the opposite defensive need: to be independent, self-supporting'. In his view those associated with the anal level are 'the wish to be good, not to be resentful, hostile, defiant, but to be loving, not to be dirty but to be clean, etc.' He characterizes the aspirations associated with the phallic phase as 'the wish to be strong, superior, great, secure, not to be weak and insecure'.

Jacobson (1953) conceptualizes the variables on which self-esteem depends in somewhat different terms. In her theoretical model, pathology in one or another of the superego, the self-critical ego functions, the ego ideal, the ego functions and the self-representations may be reflected in fluctuations of self-esteem and of mood. She implicitly concurs with Knight (1953) in his suggestion that a 'one-sided, libidinal theory of human functioning... needs to be supplemented extensively with the findings of ego psychology'. She has therefore studied and written about depression in this combined 'instinctual' and ego psychological sense.

SUMMARY

This paper has concerned itself with certain aspects of various theoretical models of depression. A review of the literature has indicated that there has been insufficient awareness that not all patients who describe themselves as 'depressed' are depressed in the same way. There is now ample evidence that the term 'depression' covers a variety of affective states which differ not only overtly, but also subjectively. Patients who are depressed experience and manifest and describe their depressions differently. The subjective affective state of the guilty, self-reproachful agitated involuntional patient, for example, is considerably different from that of the hopeless, futile, schizoid adolescent although both of these may describe themselves as depressed.

Many of the previous formulations of depression and of the depressive character

structure are simply not comprehensive enough to do justice to the variety of clinical types. Depressed patients are to be found not only among those who are excessively dependent for self-esteem on external narcissistic supplies; nor on the other hand are depressed patients to be found only among rigid over-conscientious perfectionists who expect the impossible of themselves. The spectrum is not nearly so narrow.

This relative multiplicity of depressed states—associated in some instances perhaps with private biases on the parts of the authors—has led to a variety of psychodynamic formulations and conceptualizations of the depres-

sive reactions, each with partial validity but with only too many of them implicitly designed to have universal application. For different writers 'depression' has not only different components but also different purposes. For one author it is, in essence, emptiness and loneliness; for another it is rage and guilt. For one observer it is a passive consequence of having sustained a loss in self-esteem; for another, it is an active though distorted attempt to undo this loss.

A more widespread awareness of the complexity and the variety of the depressive reactions will perhaps give rise to less dogmatic and more sophisticated theoretical models.

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TRAINING POST-GRADUATE STUDENTS IN SOCIAL CASE WORK*

By ENID BALINT

Social case workers who seek post-graduate training present special problems. Although they have a considerable knowledge of psychotherapy, called in their case 'case work technique', gained partly during their undergraduate training and partly in their jobs, surprisingly they are uncertain about how much of their knowledge they are entitled to use in their work. Moreover, even when they do try to apply their knowledge their results seem often to be disappointing and seldom clear-cut and decisive. They cannot decide whether this is because they are inadequate people, or because the techniques taught to or picked up by them are faulty, or because there is something intrinsically wrong, almost sinful, in the use of anything so near to psychotherapy by social workers. They often become anxious and their work is patchy and uneven. On the other hand they sometimes seem over-confident and can only admit to an overall need for help from someone who is sufficiently distant from them to be idealized and venerated. When they do find some post-graduate training a feeling of failure often develops, and though they do not blame their teachers for the failure they sometimes imply that the teacher could have helped them if he had cared to do so.

The teachers too are puzzled by the failure; they feel they have something to teach although they find it difficult to see exactly what it is. If they do define it their definition sounds weak, obvious and full of platitudes, and they feel sure they have left out the most important part. Even if they feel they have succeeded in their teaching they find that they are unable to understand what it was they did which contributed to the feeling of success in one group, in contrast to other attempts which ended in a feeling of failure. They make lists

for themselves of what their particular method aims at; for instance, it enables trainees to understand what is going on in the interview; or in the transference; or even in the counter-transference; it increases the trainees' toleration of unusual forms of behaviour in themselves and their clients; or it teaches them when and how to give interpretations. Or they try to explain what happened in the course in terms of their understanding of individual students or of the cases that were produced for teaching purposes. None of this, however, seems quite adequate.

The question that remains to be answered is how and why case work skills were successfully taught to one group and not to another, and what in fact was taught. Was it indeed the course that brought about the apparent difference in the workers, or has something else intervened? It could be said that it is some training experience which makes all the difference. But how, and why, and in which case?

In our experience in arranging courses for general practitioners at the Tavistock Clinic, although we encounter many problems they are of quite a different kind. Doctors are trained to treat ill people and have no doubts about their rights to approach illness. They may elect to avoid approaching their patients' private intimate problems, but when they decide to investigate them they suffer far less than do social workers from a feeling of inadequacy and guilt. It was partly the realization of this difference that enabled me to understand the particular needs of social workers, and I am writing this paper chiefly in order to emphasize the specific needs of social workers, as distinct from general practitioners. Michael Balint and I have already written a paper (1955) on teaching general practitioners psychotherapy. In this present paper I will mainly concentrate on the special

* Manuscript received 9 March 1959.

approach when training or supervising post-graduate social case workers.

Social workers then have special problems which may be linked with their uncertainties about their function, their social status, and their own personalities. Medical training gives doctors the right to take responsibility and there is usually no great anxiety in their minds about their status or about the scope of their work. Social workers on the other hand are given an insufficiently defined framework and status and consequently the results of any course of post-graduate training are often unclear and it is even sometimes difficult for the teachers themselves to be concise and clear about what they should have taught and what their aims should have been.

What should our attitude be to these problems? Should we reassure reasonably good case workers and tell them that there is no reason why they should not do a kind of psychotherapy, that they are in fact safe, normal people, who are bound to have a beneficial effect on any clients they approach? Or should we warn them against getting too much involved in their clients' problems? What do we mean by 'too much'? If we do give them reassurance, will it really make the case workers feel safe in their work, or will they just think that we know too little about them to be able to judge? Furthermore, what technique should we teach or recommend them to adopt? Can they all be taught the same technique? Have they, in fact, enough training, emotional stability and status to do advanced case work at all?

Adopting a different point of view we might decide that it is of little use giving our post-graduate students instructions about what to do or what not to do in their interviews, though most of us have been tempted to do so at one time or another, and we have then fallen back on another method of teaching which is not teaching by instruction but by the 'case-conference method'. Here the students are asked to report on their cases either in detail, that is, by process reporting, or by giving summary reports. This method usually proves better because it does give the teacher

an opportunity to see how much the trainee can do, what his own difficulties and his strengths are; but once the teacher knows all this, what use can be made of it? Can he safely go ahead and show his students what they are doing and what they are failing to do, and if so, will the student invariably be able to use what he is taught and remedy his short-comings?

In any case, as we all know, it is extremely difficult to report honestly on one's work. How is it possible to provide a setting in which work can be honestly described, bearing in mind the two main problems of the post-graduate students which I have already referred to, first their difficulty about using their theoretical knowledge, and secondly about using themselves? A teacher must overcome the difficulties inherent in this situation and avoid some kind of deceptive atmosphere creeping into his teaching; deceptive either in the sense that the students might idealize their teacher, accept what he says as being right and do their best to copy it, or deceptive in the sense that slight falsifications about interviews are made without either the student himself or anybody else realizing it. If a deceptive atmosphere creeps in and is left unnoticed, the real problems that the trainees came to solve in their post-graduate training will be by-passed.

A further problem which must be watched is how to allow for the vast differences in personality, which means differences in techniques. It has often been said that there are as many psychotherapies as there are people practising psychotherapy, and a teacher has to allow for differences in any training group—whether he is teaching future—or experienced—psychoanalysts, psychiatrists or social workers. I suggest that the problem is to enable the trainee to find his right technique which at the same time suits his job and himself. In order to do this it is less important for the teacher to find out about the job than it is to find out about the student. The student has to be helped to get a feeling of the relationship with his clients in which he himself is the case worker and not when his teacher or an

idealized image of his teacher is the case worker. Once he has realized that this is his task he will feel safe enough to be honest with his clients, which is the pre-condition of learning about his work; not, it is true, without some anxiety, but without the feeling of falseness which makes him despair about the validity of his work.

A setting then has to be provided for the training in which the teachers can get to know what kind of work the students do, and by inference, and to a limited extent, what kind of people they are. The students too can then get to know what their teachers expect of them and what they expect of each other and of themselves. They can then gradually observe what they do and can report more honestly on their work and become more aware of individual differences, which they can welcome rather than deplore. The students must be enabled to develop a mature and realistic attitude to their own work and to case work in general if they are to take responsibility for their work. For this they need the opportunity to get to know more about themselves; both their weaknesses and their strengths. In fact, the course may have to enable them to change enough to see themselves more clearly and be satisfied that they are adequate people doing an adequate job. This kind of change cannot be achieved by reassurance or wishful thinking, but only by an honest and clear realization of individual and group strengths and weaknesses, of opportunities and limitations.

Most people unconsciously wish to be tolerated as they are in spite of the wish to be helped to change. To recognize what aspects of oneself need changing is a painful and frightening process. Nobody can face it without a good deal of strain and even pain, but if a training course is to be a really worthwhile venture a setting has to be created in which this is possible, at any rate up to a point. In order to create this setting it has to be made clear early on in a course, although seldom verbally, that though changes are expected it is not beyond the powers of any member of the group to achieve this, and that the tutors will

be in no hurry but will be willing to wait. It depends chiefly on the behaviour and manner of the tutors whether or not the group realizes that such communication is being made.

The tutor's ability to accept various approaches and different outlooks from the trainees also helps to establish the kind of setting that is needed. The tutor has to accept openly that individual differences exist, and that equally good work can be done by different methods. It sometimes seems to help the group to have more than one tutor, provided it is openly admitted that tutors too have different characters and use different methods, but nevertheless are on good terms and do not try to change one another though they recognize that the techniques of each of them are limited.

With these problems in mind I would like to describe some post-graduate teaching which has been undertaken during the past ten years in groups of between five and twelve senior case workers. Each group consisted of members who were thought to have approximately the same case work background. Thus, for instance, in some groups all were staff members of one agency working together in one office, in others the workers were recruited from several offices, but each group consisted of members of the staff of the same organization.

The training itself was undertaken by senior case workers belonging to the Family Discussion Bureau (a specialized service of the Tavistock Institute of Human Relations) to whom I am indebted for permission to use the material in this paper. Their experience is largely in working with clients with marital problems. Their own post-graduate training was undertaken by psychoanalysts. Only case workers who were actually working at the time of the training in responsible positions were taken on for training. In this paper the teachers will be called 'tutors' in contrast to the 'trainees'. The training sometimes took place in the tutor's office but usually in the trainees' office. They sat in an informal way round the room; the sessions lasted roughly

1½ hours and took place once a week. The tutors usually started by briefly describing their own background and they then asked the trainees to bring forward cases which they would like to discuss. The tutors preferred not to use their own cases to begin with, although they were all doing case work at the time and felt that without this constant contact with clients they could not profitably undertake the training.

The atmosphere in which the training started is less easy to describe. The first cases that were brought by the trainees for discussion were often but not invariably difficult cases that, as a rule, had been worrying the trainees for a long time. It is indeed understandable that they should wish for help with such cases. On the other hand, it was usually clear that whereas they had great hopes that the new tutors would help them, would throw fresh light on impossible deadlocks, they had often discussed similar difficulties with their own seniors before and they knew that unless a miracle occurred nothing could be done. In fact there was an undercurrent feeling of hopelessness, as well as a rather insincere manifest optimism.

We do not know if this is a general experience in similar training courses or arises out of our particular method of training. We think that very complicated feelings were expressed in this way; one could perhaps say that often one important factor was an unconscious wish by the trainees to test out their new tutors; in this case it has to be asked what the testing out meant. For instance, bringing chronic or impossible cases and trying to find out the tutors' attitude to them may express a wish to know whether the tutors tolerated such people and whether they could help them. Would the tutors insist upon curing or changing them, or would they be permissive and tolerant and leave them as they were? Or would the tutors want to get rid of them, to refuse to work with them? Responding to this test was made more difficult by the realization that sometimes the trainees expressed in this way their own unconscious needs and fears

about themselves. Could the tutors help them to change, or would they tolerate them because they wished to be tolerated whatever they were like; would the tutors recognize that the trainees were really as good as it is possible to be, as any suggestion that they needed to change was felt as hostile criticism; or would they refuse to teach them because they were the kind of people they were?

At a fairly early stage the tutors had to decide whether to interpret this kind of anxiety to the group or not, whether they should tell the group that their clients' problems were brought up for discussion because they were also the group's most pressing problems as well. Or whether to concentrate on the clients' problems and not relate them to the group's. In any case, the tutors cannot but show what kind of people they are by the way they tackle a problem or behave towards the group and, therefore, there is often no need to verbalize the trainees' anxieties about this. In spite of this, in many groups, particularly where the level of work was of a fairly intensive kind, we have found that after four or five meetings some kind of group interpretations had to be made since, in fact, the case under discussion for that day was so clearly the group itself. We have had many examples in which omitting this or failure to deal with it adequately led to failure and breakdown of the course. By their omission they created the impression that they did not understand one of the problems for which the trainees had, unconsciously, sought help and so they gave up hope.

To give an example: a group of case workers who worked together in a poor district on the outskirts of a large town had made many previous attempts to get help. Each time they started a new course they were enthusiastic but this never lasted and the teaching petered out. The senior case worker had tried various ways of improving the work in the office—she herself had sought private supervision, had changed her staff, tried new methods, and so on. By the time the Family Discussion Bureau was called in it was obvious

that the group was very apprehensive and had little hope. Each member of the group seemed preoccupied with private worries and there was little sign of any group feeling or of mutual support. There seemed to be no shared experiences, no common jokes, no local loyalties. For the first four sessions little headway was made and gradually the feeling of hopelessness became so great that the tutor felt compelled to interpret to the group that the reason why they felt so helpless was that they were in a situation where they had to form a relationship—to 'give' themselves, in spite of their apparent conviction that they had nothing to 'give', that they were useless partners in any relationship. In consequence the group was preoccupied with the dangers of making relationships, that is to say of the uselessness of the group itself. After this session the group was able to start to work and to bring cases where relationships were made and to feel more related to each other.

In another group the mutual understanding between the various members was so good that at first the group as a whole would not tolerate any of its members being criticized. Everything everyone did had to be seen as valuable and good, and the same was true for the clients. True, this group were fully aware that they were often wrong—but they could not allow the tutors to see how individual wrongs should be put right. It was only after some work which led to the recognition and acceptance of differences in people that any progress could be made. Only after they had become sure that they were not expected to conform to their tutors' standards could they consider the necessity of making even small adjustments in themselves or in their clients.

It is very important for members in a course to feel that they have the support of their own organization, and it is equally important that the request for help should come from the members themselves, that is, that they are not detailed to the course by their organization. If they have both the backing from the organization and the feeling that they are seeking help themselves, many ups and downs which

arise in a natural way during a course will be more easily tolerated. For instance, it is quite usual that by the end of the first few months a group will reach a rather alarming over-confident state; they may feel that they have found the answer to all their problems and will start using words like 'magic' to describe the results of their new technique. There may also be some hilarity and members will start interpreting the behaviour of each other. After a few more months this phase will calm down and some hard work will be done, everybody being rather conscious of the shortness of the time available in their seminars. Later still there will often be a return to depression and doubt with some criticism of the tutors for offering them a taste of something promising and then sending them away unsatisfied. Gradually and imperceptibly, however, insight will be shown that would have been unthinkable before the beginning of the course but by this time it has become so much a part of the group work that members will be unaware of it and think of themselves only as using their new skill when they are self-consciously and deliberately 'giving interpretations'.

Real changes may be shown in small and at first glance unimportant ways. For instance, in one case immediately after the first course a woman member changed her routine and saw matrimonial clients by appointment only, allocating an hour to each, which she had said would be impossible before because of pressure of work; moreover, she shared cases with her colleagues as is the routine at the Family Discussion Bureau, whereas before she saw both partners herself. All this did not seem to take up any more time than the old haphazard arrangement and achieved better results and gave greater satisfaction to the staff. It appeared also that many members redecorated their offices, or rearranged their interviewing rooms, and this seemed to be not just an end in itself but an expression of a different outlook. Remarkably, all these developments were generously supported by their organization.

Courses have also been arranged for groups

of voluntary case workers, few of whom saw their clients more than once or twice and who had until they sought help been content to give some simple advice and instructions. With increasing insight these case workers began to keep in contact with their clients for much longer periods and tried to understand something about unconscious mechanisms. They were all willing to work very hard and to attend any kind of courses we could recommend. The members themselves were good people and many had some professional training before taking up as voluntary workers. In spite of this we found their lack of professional background and of professional responsibility rather alarming. We feel much safer when trying to teach people when they are working within the discipline of a regular job and having daily contact with a group of colleagues; that is to say, people with some status and some security in the sense of a salaried job. We found that when this is absent the claim to be a social case worker at all depends too much on the belief of each person in his own emotional stability, which is a terribly fragile foothold and one which we dare not shake too much.

Individual workers are certainly helped by good personal relationships outside as well as inside the group. In fact workers need to feel good enough and acceptable people, if they are to venture on work which is bound to be felt for a time to be just too difficult for them or which is just beyond what they feel to be safely within their powers. In due course the training group itself becomes a safe place from which to venture forth; however, it is equally necessary for the training group to fail at times, and if there is no other safety for the trainee except the training setting itself, then periods of failure cause too great strains; in other words, although the members may feel safe together and therefore cling to their group, little life or spontaneity can be tolerated, and the whole venture becomes dead.

The tempo and the intensity of our teaching are automatically regulated by the ensuing feeling of safety within the group. No pre-

arranged plan is ever made by the tutors about how fast to go or how much anxiety to raise, but it is their job to keep the strain at the right level; that is to say, to see that the group atmosphere is not too permissive and too anxiety-free or too tense at any given time. If tension is allowed to rise when there is no safety in the group as a whole, individual members will become anxious about themselves as individuals instead of about their clients or about their work; on the other hand, little work can be done if no anxiety can be raised.

Moreover, it is important to establish in the groups an atmosphere of mutual responsibility between trainees and tutors. We do not mean that no primitive behaviour or relationships are tolerated in the group, but the primitive relationships are felt mainly to be useful against a background of adult responsibility. As a rule, only case workers in jobs are accepted for training; that is to say, only fully responsible experts in their own field. Moreover, the tutors are, as a rule, not experts in the special fields from which they recruit their trainees. In one respect therefore the trainees are knowledgeable and responsible case workers and in another they are minors looked after by responsible adults. This dual role of the trainee has important repercussions and is in our experience a partial, although not a complete, safeguard against the danger of the development of very childish dependent attitudes in the groups by which the trainee's confidence in his own responsibility may be jeopardized. The tutors in fact have authority only in their own special field but not in any other; the trainee is constantly reminded that he may claim and must accept a similar status in his own field. Both can learn from the other but there must never be any question about where responsibility lies.

In our training it is relatively easy to demonstrate this mature atmosphere on which we lay so much importance. The main material for discussion in the field of the Family Discussion Bureau consists of cases of marriage breakdown; what we are working with are adult, not infantile, sexual relationships.

Similarly it is one of our aims—in our case work as in our teaching—to foster an adult atmosphere of mutual responsibility between ourselves and our clients. Our main concern is to help the marriage partners to mature, so as to be capable of adult sexual relationship. It may therefore be easier for us, than for other case workers, to establish this same atmosphere in our training groups. Furthermore, our aim is to help our clients to see what each of them contributes to the difficulties in the marriage and what each may do about the solution. Similarly with our trainees, our aim is to help them to see what they contribute to their professional difficulties, what they may do to alleviate them there as well as in their training groups. In fact three tiers of this responsible relationship may be studied in the training groups; first, the relationship between the tutors and their trainees, secondly, between the trainees and their clients, and thirdly, between the clients and their marriage partners. We stress again that we do not wish to give the impression here that we never deal with primitive attitudes at all; this obviously is far from true, as they inevitably form an important part of every mature relationship.

Social case workers are found very often in organizations in which many professions are represented; for instance, they have to work in hospitals with doctors and nurses, in government departments with civil servants, lawyers and administrators, in child guidance with doctors and psychologists, and so on. It is therefore particularly difficult for them to know exactly what their status is or how much responsibility they are expected to bear both for their work and for their set-up. Very often, too, more than one social worker is responsible for one client and this divided responsibility causes additional difficulties when some psychotherapy is needed. It may even turn out that more than one social worker is doing

psychotherapy with the same client, which has the effect of making the social workers doubtful about the validity and usefulness of their work. In post-graduate training therefore it is particularly important for the tutors to help the trainees to become aware of their real role and function and realize their limitations, but develop a realistic confidence in their work.

SUMMARY

Those who attempt to teach advanced social case work are frequently hampered by the lack of clarity in their own minds as to their own function and as to the function of their students. The results of their training are therefore unclear. On the one hand they may idealize case work and feel there should be no limitations to what they should be able to teach, and on the other hand they feel doubtful in their own minds about how much responsibility social case workers should be allowed to take. In studying these problems with the Family Discussion Bureau I have found that social case workers can be helped, using the group methods described in this paper, to learn enough about themselves and their roles as social workers to be able to take responsibility for their work and to accept their shortcomings and strengths without too much frustration or loss of pleasure in their work, and without the need to feel omnipotent. During this kind of training the trainees learn what they as individuals can and cannot do, and also, no less important, what are the limitations and possibilities of psychotherapy as a whole.

Similar problems, of course, occur in the training for every kind of psychotherapy from psychoanalysis to the most unambitious supportive work. The special status of social workers is their somewhat undefined and highly varied role, which throws light on problems discussed in this paper.

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TOWARDS A CLARIFICATION OF THE 'THERAPEUTIC COMMUNITY' CONCEPT

By MAXWELL JONES*

What is meant by the term 'therapeutic community'? The words themselves suggest a group of people assembled for the purpose of healing. Ordinarily this would occur in any hospital unit—ward, block, or other integrated treatment organization. The therapeutic community is *distinctive* among other comparable treatment centres in the way the institution's total resources, both staff and patients, are self-consciously pooled in furthering treatment. This implies above all a change in the usual status of patients. In collaboration with the staff, they now become active participants in the therapy of other patients, and in other aspects of the overall hospital work—in contrast to their relatively more passive, recipient role in conventional treatment regimes.

Hospitals, like all institutions, have subcultures that distinguish them from the surrounding community, and within the hospital are sub-groupings of each which have their own characteristic patterns of ideas and behaviour. Being absorbed into the life and work of the hospital involves accommodating one's own ideas and behaviour to the norms within the institution. Deviation brings sanctions that make absorption into the group more difficult or impossible. The usual situation in hospitals is that patients have some incapacitating illness that makes it difficult for them to accommodate themselves to ordinary life. The hospital, with its relatively simple social organization, offers a sheltered haven for such people. But patients differ in the degree to which their incapacities contribute to deviant behaviour and the extent to which the causes of illness are apparent. For example,

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a patient with a broken leg has both a great degree of ambulatory incapacity and an obvious manifest cause. His lying in bed is not considered deviant in hospital because the norms governing hospital life are geared to incapacities of this kind. A psychotic, who might be less incapacitated in terms of actual physical mobility, might still show disruptive behaviour patterns that make his adjustment to the social life of the ward difficult. His obviously disturbed mental state provides a relatively manifest cause, but the problem of what kinds of norms should govern ward life for such patients is a major preoccupation of modern psychiatric hospital reform and by no means clearly established. The tendency, in spontaneous interaction with such cases, might be toward disapproval and rejection, especially if his behaviour is not easily understood or justified nor associated with any manifest disturbance in mental state. This is the situation with certain atypical schizophrenics, many 'psychopaths', hysterical personalities, some paranoid patients, etc. Staff generally find it more difficult to understand and sympathize with these cases whose behaviour lacks manifest cause. The tendency is rather to see them as 'bad', or 'anti-social', and to shun, avoid or become angry with them. Amongst fellow patients the new patient of this type may call forth a variety of responses. He was not sent for by them, nor were they asked to assume responsibility for his induction to the ward; he has taken the bed of a recently discharged patient who had become in some degree assimilated and whose neighbours may not welcome the stranger in their intimate presence. They are not yet familiar with his behaviour patterns, and may tend to be apprehensive about what new problems will be raised by his as yet undefined

interpersonal difficulties. Reactions to this situation will vary enormously depending on many variables, e.g. the current social climate of the ward, the personality attributes of the new patient and those of his fellow patients, and so on.

In his relationship with the doctor, the patient seen in individual interview might show a rather special pattern of behaviour that might or might not be consistent with his ordinary behaviour. While the doctor in private interview might get many aspects of the overall problem through referral notes, retrospective accounts of the patient and observations in the immediate situation, these are all subject to distortion and in some circumstances may be grossly inaccurate.

There is inadequate provision in most psychiatric hospitals for communicating information that would enhance knowledge of patients' ordinary social behaviour. This constitutes a serious, sometimes a crucial, loss of treatment opportunity. The doctor may exchange limited information with the charge nurse, but the canons of confidentiality inhibit even this channel. It is rarely that the informal contacts of junior nursing staff and patients are made a part of the explicit treatment programme.

The concept of a therapeutic community implies that the three principal lines of social interaction, (a) patient and his peers, (b) patient and doctor (with partial involvement of charge nurse) and (c) patient and junior nursing staff, be developed and integrated into a coherent treatment effort.

(a) Patient and his peers

The patient's relationship with his peers provides rich potentials and serious problems in this integration effort. They are his constant companions, and it is virtually impossible for them to avoid intimate face to face sustained contact. On the surface it would seem that many of them would be so handicapped by their own illness that they could neither wish nor be able to deal with the troubles of

others. In addition, their knowledge of psychodynamic theory is so little in comparison with doctors and other trained staff that their contributions, even assuming motivation in that direction, would seem of doubtful value. On the other hand, their information about the patient and the vagaries of his behaviour tends to be unexcelled in the hospital.

Whether communicated to the staff or not, the ward life goes on and interpersonal difficulties are constantly emerging. Ordinarily the ward has to deal with its problems as best it can with the occasional help of the nurses. In a therapeutic community this material is made available to the various group and community meetings with trained staff present. Here the patients have an important part to play in treatment, guided by their intimate knowledge of one another and their intuitive appreciation of one another's psychological difficulties. Their lack of training, however, may lead to considerable frustration and the accumulation of tensions unless skilfully handled in situations organized for the constructive use of these communications. By participating frequently with the staff in such group and community meetings some patients can acquire considerable skill in group therapy, and to some extent apply it even in informal contacts outside therapeutic groups.

(b) Patient and doctor

At the other extreme are the doctors and other highly trained technical staff. While their psychotherapeutic skills are the highest available in the hospital, their access to the spontaneous interactions that provide the most vital and valid current information tends to be limited. In those cases where the consulting room relationship is the only communication channel through which particular patients can operate profitably, some advantages may be gained in preserving conventional two-person therapeutic contacts. But even here the behaviour may be unrepresentative enough to be of limited use in short-term psychotherapeutic practice.

(c) *Patient and junior nursing staff*

Between these two extremes are the junior nurses, less trained than their seniors yet more so than most patients; less involved in the ongoing intimacy of face to face ward relationships than the patients, yet more so than the doctors.

For the junior nurses the problem of education should if practicable be linked with that of providing information for the doctors by establishing daily tutorials in which psychodynamic theory is applied to current ward problems. This, in turn, rewards senior staff by assisting both with the problem of handling ward affairs and with the problem of maintaining junior staff morale. The daily contact guards against excessive staff alienation from the informal ward culture (Caudill *et al.*, 1952; Stanton & Schwartz, 1954; Parker, 1959).

The problem, it appears, is to so arrange a distribution of technical skill and of vital information that treatment can be provided wherever it is appropriate by whoever is at hand. How this can be worked out will vary from hospital to hospital and will depend in part on the personalities, ideologies, and training of the staff, as well as the nature of the clinical problems being treated. Our own method of treating 100 adult patients of both sexes with severe personality problems, including borderline psychotics, by community methods (Jones *et al.*, 1953; Jones, 1956, 1957) is only one of innumerable possible patterns and our treatment ideology and social organization are constantly changing. Wilmer (1958) has used a similar approach in an admission ward in a mental hospital. We have moved progressively in the direction of patient participation in practically all treatment and administrative activities. This implies an increasing limitation of individual psychotherapy and growth of community* and group treatment situations. There have been profound changes in the overall social organization from a predominantly hierarchical one, as found in most hospitals, in the direc-

tion of a democratic, equalitarian one (Rapoport & Rapoport, 1957). Along with this have been sweeping changes, occurring gradually over a period of twelve years, in the patient and staff roles. The tendency has been for the patient's role to become progressively closer to the roles of the staff, with whom they share most decision-making in the area of their own interests and participate in most treatment situations. In line with this trend many new values come into being: thus the concept of privileged communication has been largely given up by the staff and been replaced by the norm of sharing even the most intimate material with the group. The doctor or anyone else reserves the option of feeding back any information to the group if it is thought that he will help the patient by doing so. Such values, however, have a varying degree of acceptance by the community and may be believed and acted on by most people only during a 'good' phase of the Unit's existence when people's images of others are predominantly helpful and understanding. Even during a 'bad' phase the idea will be kept alive mainly by the more stable and permanent staff members and reappear in practice with the return of a more favourable social climate (Parker, 1959).

When the broad outlines of a therapeutic community are considered it is clear that the change in social organization in the direction of a more democratic equalitarian structure will result in many cultural changes. Thus the greater participation of patients in treatment of other patients will raise questions about education and support which have much in common with the problem of training the student nurse for a somewhat similar therapeutic role. Here we are raising a

* By a community meeting we mean a meeting of the entire patient and staff population. This occurs daily from 8.30 to 9.45 a.m. In addition there are numerous smaller groups such as doctors' treatment groups, ward groups, workshop groups, new patients' groups, departure group, visitors' group, family groups, etc.

cultural concept which may have considerable significance as a specific treatment method. It is our belief that the establishment of a therapeutic community as described above forces one to consider how the body of knowledge, beliefs, values, etc. which are held in varying degrees by staff and patients alike is actually therapeutic. In other words patient-patient interaction or nurse-patient interaction has now to be analysed in much greater detail than formerly when little attention was paid to what went on at this relatively untrained level. The change to a therapeutic community may result in the establishment of therapeutic groups taken by the doctor, and larger ward or community meetings where social and local administrative problems can be raised. In the former, 'treatment' is still largely controlled and directed by the doctor, and the same control applies in a lesser degree to the ward meetings. We can carry this process further so that patient participation is given an even more prominent place in formal treatment, e.g. in workshop discussions where no doctors are present and the personality difficulties which emerge in the actual work situation are discussed by the patients in the presence of the instructor. Alternatively more attention may be focused on the informal groups which occur amongst the patients after 'lights out' or in the living rooms, etc. Here much active 'treatment' may go on, but such situations may equally well be anti-therapeutic. The aim of a therapeutic community would be to encourage the feed-back of relevant material to a more formal group with trained staff present. Enough has been said to illustrate how by changing the social organization of a treatment unit in the direction of a therapeutic community many new aspects of social interaction come to be seen as potentially therapeutic. But how far can these various areas of social interaction be brought together into something approaching a treatment methodology? To put it another way: are we yet in a position to attempt to mobilize the social forces present in any community in such a

way that they can constitute a treatment method, comparable with, say, what has been achieved in group psychotherapy? It would, I think, still be premature to do so, but our knowledge in this area is growing steadily. All that can be attempted at present is to study in more detail some of the cultural concepts which emerge when, as in a therapeutic community, people begin to examine their ideas, values, beliefs, etc. and self-consciously attempt to modify or reorganize them for a therapeutic purpose.

DISCUSSION

It has already been pointed out that all hospitals have their own distinctive sub-cultures and that within the hospital are sub-groupings each of which have their own characteristic patterns of ideas and behaviour. The cohesiveness and distinctive qualities of these sub-groups will be related to the social organization of the Unit and the extent to which ends and means have been clarified. Even more, it will be related to the extent to which the patients and staff believe in its methods, values, and aims. How often do we find a psychiatric ward where a frank evaluation of the treatment aims and methods in both staff and patient groups is traditional? Each group will have its own characteristic ways of doing this, but mostly it goes on in cliques and status groups and overall communication is disapproved of. Clearly there are very good grounds for this, as the highly trained doctor may not feel that he can usefully discuss problems of lobotomy, sedation, psychotherapy, etc. with the patients in the ward, or may prefer to handle such problems on an individual basis with the patient whom it concerns. The same problem of communication and sharing is apparent in each of the status groups within the staff.

If we consider communications which are 'privileged' the problem becomes even more complex. Medical ethic and patient expectation usually imply that a confidential communication between a patient and his doctor

is a matter for the doctor alone or in special circumstances other members of the treatment team. The law-courts recognize such privileged communications, although in exceptional cases this immunity may be questioned. However, it can be argued that most information regarding treatment and administration can with advantage be shared by the community. Even in the case of privileged communications it is surprising in our experience how readily the culture can change and the principle of sharing confidential material with the community be accepted. This is all the more surprising as many of our patients have been in prison where, at least in restrictive prisons, an entirely opposite attitude towards communication prevails. In prison the fear is that personal information will be abused or even lead to punishment; in any case no one is to be trusted, and the appearance of good behaviour will lead to a remission of the prison sentence. This hostile attitude towards authority and even towards the peer group tends to be circumvented in a therapeutic community. The group and the community are there to be confided in rather than the doctor in a confidential setting. In individual treatment the doctor must in part at least be trusted—no matter what the vagaries of transference may be—if the patient is to be helped; so in a parallel way the group must come to be used in a therapeutic community. This means in practice that much confidential or semi-confidential material is fed back to the group or community meetings, because this is seen as a way of helping in the individual's treatment.

Informing is allied to this problem of communication but calls for an even greater change of value judgement for most patients. 'Telling tales out of school' seems to be alien to most of our patients, at least on admission, and implies some gain for the informer in that

he gains approval from an authority figure, at the expense of the other person who suffers punishment from authority. Only the most sophisticated and insightful patients can accept such a change in the culture that informing is now seen as potentially therapeutic. Even when this social attitude is accepted it may still be difficult to act on, because it may be misconstrued by at least part of the community and expose the individual to later reprisals. These remarks are based largely on my experience in treating patients with severe character disorders, in a therapeutic community developed to meet their particular treatment needs. The growing literature on the subject of therapeutic communities (Wilmer, 1958; Greenblatt *et al.* 1955; Caudill, 1958) bears witness to the fact that similar treatment methods are being developed in some mental hospitals.

I have visited several of these hospitals in Britain and the U.S.A. where they were treating a predominantly schizophrenic population in either acute admission or long-stay wards. It seemed to me that the schizophrenic patient was being helped to view his 'badness' and destructiveness in a setting where his fears were not only acceptable but both patients and staff were eager to help. In this way the patients were helped to deal with their 'bad' selves by incorporating good objects and so modifying their own destructive super-egos.

In conclusion I would like to stress that, in my opinion, the concept of a therapeutic community has values in the spheres of nurse and staff training generally, in opening up communications between all hospital personnel and patients, and in consequence of these factors improving patient management. It may and I believe does amount to a specific treatment method, but this awaits further development and proof.

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SCENT AND THE SEXUAL OBJECT*

By JOAN FITZHERBERT

This paper is a sequel to that by Pratt (1942) entitled 'Notes on the unconscious significance of perfume', to which the reader is referred. In his paper, Pratt derives the aphrodisiac qualities of scents from the fact that they contain aromatic substances present in faeces, axillary sweat, and urine.

Speaking of the 'action at a distance' of smells, Pratt mentions the footnote in *Civilization and its Discontents* in which Freud says: 'The organic periodicity of the sexual process has persisted, it is true, but its effect on mental sexual excitation has been almost reversed. This change is connected primarily with the diminishing importance of the olfactory stimuli by means of which the menstrual process produced sexual excitement in the mind of the male.' And further on: 'The diminution in importance of olfactory stimuli seems itself, however, to be a consequence of man's erecting himself from the earth, of his adoption of an upright gait.' (1929).

From this it is clear that Freud was equating menstruation with 'heat' in animals. (The fact that the one is not the equivalent of the other was not discovered until the 1920's.) 'Heat' in female animals is the period during which they are sexually excited and will allow, or even incite, the male to mate with them. 'Heat' occurs at the time of ovulation, and in this way reproduction is secured. On the other hand, menstruation, which occurs only in women and in some of the primates, does not coincide with ovulation, but on the contrary sets in fourteen days later, if the ovum has not been fertilized, and is due to the shedding of the endometrium, or lining membrane of the womb. Conception is not possible during this period or for some days afterwards

(this latter being the so-called 'safe-period' for intercourse), the ovum having already passed out of the woman's body. From the point of view of reproduction and the preservation of the species, therefore, women should be sexually in their least excited, and exciting, phase during menstruation. It is obvious from this that when in the course of evolution menstruation first developed in early woman or our prehuman ancestresses, natural selection would 'breed-in' those whose mothers remained sexually 'on heat' at the time of ovulation, and that females who were particularly sexually excited during menstruation would fail to reproduce, so that this latter tendency would die out. For the same reason, moreover, natural selection would favour the development of men who ceased to find the smell of blood on the female genital region sexually stimulating or who even found it repellent.

The smell of the decomposition products of stale blood could readily suggest some of the odours occasionally present in faeces, and so increase disgust for both substances. All this provides a much better explanation of the 'organic repression' which Freud postulated as underlying the 'taboo of menstruation' than he was himself able to give. At the same time it confirms his observation that a former sexual stimulus has come to be 'almost reversed', and that this was connected with olfactory perceptions.

It seemed to me that Pratt had failed to account for that 'particularly nostalgic quality of memories aroused by odours' which he himself mentions, in spite of his own reference to 'the biological fusion of the sense of taste and smell', and his penetrating remarks about 'the in-drawn breath that accompanies the perception of odours, themselves experienced "inside" the body', and of this favouring 'the

* London. Manuscript received 19 November 1958.

phantasy of the incorporation of the object by means of its odour'.

My first notion in seeking an explanation was that the odoriferous substances in human faeces, which in the infant are derived from the milk itself, served to recall unconsciously the taste of that milk and all its associations with the first feeding situations. On inquiry however I was informed that really clean fresh milk, human or other animal, has seldom any detectable odour, and that its taste depends chiefly on the salt and milk-sugar content, though to a lesser extent also on that of its fats and proteins. This did not seem to support my hypothesis, although my informant added that even slight bacterial or enzymic breakdown of the constituents of milk resulted in the production of odours, mainly due to volatile acids formed by the decomposition of the milk fats. Of these, butyric acid, which is found in butter, sweat, and faeces, would obviously be important. With present-day standards of cleanliness in infant feeding, however, even this seemed too slender a foundation for such a hypothesis.

To digress for a moment: I do not know of any theory which seeks to explain, as opposed to describing, the occurrence of the anal stage in human psycho-sexual development. Is this stage simply due to the infant's pleasure in his developing ability to control his evacuations, and the fantasies which this allows, and is his delight in the smell of his excreta due to the association of this smell with pleasurable defecation after a satisfying feed and with the smell sometimes perceived in connexion with his mother herself (that is, when she passes flatus)? Or is the occurrence of the anal stage due primarily to the infant's partial recapitulation of some of the psychological stages of our long evolution? Does it represent a vestigial psychological structure corresponding to the time when our remote ancestors were animals with cloacae? (That is, with a joint opening of the uro-genital and alimentary systems, which arrangement is characteristic of all vertebrates except the mammals, and is found also at one stage in the development of

the human embryo.) This may seem extremely far-fetched; but first, the cloacal stage is a much later one than that in which our ancestors had gills and yet human foetuses still pass through a gilled stage, and secondly, the whole of infantile sexuality is itself, according to Freud (1939), a psychological recapitulation of an ancestral stage at which we matured sexually when about five years of age.

To return, however: I then recalled two facts about cats which I had noticed. First, that when purring and showing affection, a cat will thrust its nose into its human admirer's armpit, if the latter smells in the least sweaty. Secondly, that the abdomen of a nursing mother cat sometimes exudes a powerful smell like that of human sweat. Since women sweat between and under the breasts as well as in their axillae it occurred to me that perhaps it was the smell of maternal sweat which became closely associated in the infant's mind with his first feeding situation.

Yas Kuno (1956) describes two main types of sweat glands, the apocrine and the eccrine. They occur only in some species of mammals, and the two types have different functions. In animals, Kuno says, 'the apocrine glands are commonly present in the general skin covered with hair, while the eccrine glands are restricted to the hairless regions of the skin, for example on the pad of the cat's foot'. Sheep and horses, both of which sweat profusely, are thought to have no eccrine glands, whereas in man the eccrine glands are distributed over the whole body surface, while the apocrine glands are confined, in Europeans, to the armpit and the area round the nipples in men, though sometimes they are also found on the skin below the navel, while in women they occur not only in all these regions but also in the labia majora and the mons pubis. The sweat secreted by the apocrine glands 'consists of many organic substances with more or less odour', that of the eccrines is a very dilute fluid which is practically odourless (its function, of course, being concerned with the regulation of body temperature). Some other facts are worth mentioning, and these

are that the average baby begins to be able to sweat from its eccrine glands between the third and fifth days of post-natal life, whereas the human axillary glands do not begin to function until 'a few years before the onset of puberty', that the characteristic apocrine smell does not become noticeable until about puberty, that these glands 'undergo some cyclic or temporary changes concomitantly with menstruation or pregnancy, and lose their activity in old age'.

As to the function of the apocrine glands, Kuno says: 'On the human axilla, sweat can be produced by emotional stimuli as easily as on the palms and soles... this sweat is of use for sending forth the axillary scent.' Again: 'It is well known that the scents of most mammals increase remarkably during the breeding season and excite the sexual impulse of the other sex... In human beings... the sexual attraction seems to be almost the sole effect which still has any significance. In this attractive power, the axillary scent in man seems to be superior to the scent of the sexual organ or of any other parts of the body.'

He adds: 'Now that the human olfactory organ has been brought up by standing to a height roughly five feet from the ground',... the axilla 'is in an advantageous position, and its scent acts far more successfully. The axillary scent has therefore developed in man.'

What substances are actually responsible for the smell of apocrine sweat, Kuno states it is still difficult to say, but he adds that certain fatty acids probably play an important role, and that of these valeric, caproic, and caprylic acids have recently been identified (as has butyric acid). Actually, fresh clean apocrine sweat itself, like milk, has no smell; its odour too develops as a result of decomposition by bacteria.

In view of all this, I would now suggest that a baby feeding at its mother's breast with the apocrine glands of the skin surrounding her nipple within a few millimetres of its nose must inevitably come to associate the smell of her sweat with the taste of the milk, and with the comfort and satisfaction of the whole nursing

situation. Even a bottle-fed baby is usually held on its mother's arm, near her nipple and axilla, and all babies lying on a woman's lap, even that of the cleanest mother, probably smell if not the apocrine scent from the skin below her navel, at least that from her genital region, as well as the odours of her vaginal secretion and her flatus.

Until very recent generations, when bodily cleanliness became something to be cultivated, babies must have experienced not only the smell but also the taste of the decomposition products of milk on their mother's or wet-nurse's nipples, and on the clothing covering her breasts, so that one might reasonably go even further, I think, and deduce that the characteristic apocrine smell was evolved, 'bred-in' by natural selection, precisely because of its resemblance to that of the breakdown products of milk, such as butyric and other volatile fatty acids. The association of these smells with the breast would help to guide the hungry baby's nuzzlings, and lead the straying toddler back to his nursing mother, and the reinforcement of the smells of her stale milk by those of her apocrine sweat would therefore assist in ensuring the survival of her offspring. Lest this seem far-fetched, I will quote some remarks of a teacher of psychotic children, Miss G. D. Clark (1958). She says: 'Some of our children on admission appear to have a complete lack of either painful or pleasurable sensations from outside in both the emotional and the physical sphere. There seems no differentiated response to bodily stimuli as far as one can judge from the appropriate facial expressions... They have no apparent sensory perceptions except perhaps that of smell. Certainly the sense of smell appears to be the only one used for pleasure. They often show awareness of new smells, one's hair and skin and so on. I find that if I use a new tablet of soap I am sniffed all day by successive groups of children.'

Clark also mentions that these children sniffed with obvious pleasure at the 'stink lorry' which emptied their cesspit. It seems to me therefore that yet another contributory

factor to the development of an anal phase might be the child's transfer of some of his olfactory interest in his mother from the smell of her lactating breasts to that of her anal region, at about the time his period of breast-feeding ceases and he begins to walk. Such a transfer would help to maintain that attachment to her person which preserves him from the dangers of straying too far from her, just as the smell of her apocrine sweat would do. It is to be noticed that the toddler's nose is at about the right level for this when he first 'erects himself from the earth', to use Freud's phrase, so that it may be we have here the explanation of the survival of an anal stage in human psycho-sexual growth.

To return to my argument: a woman whose sweat had a smell reminiscent of the odour produced by her milk would, because of it (if I am right), be sexually a more exciting object, even to adults, than her unscented sisters, and therefore her chances of reproduction would be increased, so that by this means too the trait would be 'bred-in'.

What I am suggesting in this paper is that, in Pratt's words, 'the particularly nostalgic quality of memories aroused by odours' is due to their unconscious association with the earliest oral satisfaction and its accompanying love relationship. This would give an additional explanation of the urge to draw in the breath deeply on smelling a sweet scent, to part the lips while inhaling, and to

take in, 'drink in', and incorporate something of the scented object.* The closing of the eyes not only recalls the infant's falling asleep satisfied at the breast, but may also be a recovering of what Lewin (1953) describes as 'the dream screen', the expanse of smooth skin of the warm lactating breast.

If what I have suggested is correct, the people one would expect to be most susceptible to scents would be, I think, those who had had some good experience at the breast, enough to have confirmed to them the existence of such comfort and pleasure, but not enough to have satisfied their emotional needs in the oral phase. It might be interesting in this connexion to investigate the effect of having been bottle-fed on people's later response to smells.

Lastly, I would point out that in many kinds of animals the sexual object and food are both hunted by their scent, and that in the oral stage of human infancy these two objects are one and the same.

Pratt himself quotes (though without pursuing the suggestion they contain) some very apt verses from Donne:

As the sweet sweat of Roses in a Still,
As that which from chaf'd Muskat's pores doth
trill,
And as th' Almighty Balm from th' early East,
Such are the sweat drops on my Mistris' breast.

* Since this was written, brassières containing a scent sachet have appeared in the window of a London shop.

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ON THE DYNAMICS OF REPRESSION, AND EGO-SUBORDINATION

BY IMRE HERMANN*

An essential mark of psychoanalysis is that it describes psychic phenomena in terms of reciprocally interacting biological forces. The terms cathexis, abreaction, instinctual drive, self-regulation, repression all presuppose the existence and interplay of such forces; but we look as yet in vain for any measurements of them carried out by any sort of measuring instrument, or based on comparative research. We have to be content for the present with more moderate pretensions, and to seek opportunities for comparing the relative intensity of such forces. This paper concerns the comparative strength of repressive forces and of forces repressed.

It has been observed experimentally that if while he is talking a noise drowns the subject's voice, he immediately raises his voice volume, even though this may not help the speech to be heard any better (Hermann & Fónagy, 1958). A homeostatic reaction such as this demonstrates an interplay between two opposing forces, and serves as a useful analogy for the mental events which ensue when one mental force gets blocked by another. If a powerful impulse is blocked on its way to consciousness, its energy does not diminish, but on the contrary its driving force grows; and instead of using the direct route to consciousness, the impulse may choose a circuitous path; so that what I have called the 'whirl-like process' (Hermann, 1935) sets in. The over-proliferation of impulses that results is of course in accord with the phenomenon of over-production stressed by Rado (1956).

The study of the strength of repressed impulses is of major importance because of views about the ubiquity of repression. For Madison

'repression (in the sense of unawareness) is not *one* of a number of defences, it is the essence of *all* defences' (Madison, 1956); while Schilder regards all cases of unawareness, even when this affects but unimportant impulses, as repression (Schilder, 1928). If, in repression, *the strength of what is to be repressed will be reinforced*, it follows that the repressor must increase his watchfulness, and that the intensity of the repression will in all likelihood become enhanced, unless it was very severe to begin with.

The idea of reinforced repression now arrived at allows further understanding of an observation commonly discussed by various writers. Montaigne's essays provide two good examples: 'Socrates loved virtue deeply, and therefore renounced all efforts so as to be able to acquire it with more ease'; 'What one can do without effort and naturally, one cannot do under constraint and on command'. The same phenomena are noted in more scientific terms by French: 'excessive tension tends to disintegrate the pattern of goal-directed behaviour... the difficulty of this (integrative) task of channeling motor-discharge into effective effort will tend to increase in proportion to the amount of tension that had to be controlled or bound... if the integrative task at any time exceeds integrated capacity, then we must expect some kind of disintegration of the mechanisms of goal-directed behavior' (French, 1950). It goes without saying that repression in Schilder's sense is involved in every integrative process.

Reinforcement of the repressed is suggested—I would say illustrated—by other early observable acts or behaviour. Fascination with the forbidden, a subject often dwelt upon in analytic literature, provides an easy example. Again, analyses often reveal how, in childhood, the more the gloomy thought

* Budapest. Manuscript received 12 January 1959.

that a father or mother may die is put away, the more vigorously the idea rushes upon the child, so that he cannot help pondering on death and burial in tormenting forms. A similar dynamism can be noted where a child—or for that matter a grown-up neurotic—resolves to be very good and friendly, yet cannot help being very unfriendly instead; and when attempts to repress blushing result in more blushing than ever; and when enuresis gets worse the more the patient is put to shame, or the more he tries to control or repress the impulse to urinate.

The special class of repression provoked from outside, by parents' commands and prohibitions, which require the child to repudiate his ego, his desires and his impulses, provide no exception; here too the impulses reappear with reinforced intensity. As one patient put it, 'the psychic cellar opens up'.

The above clinically well-known facts have by themselves only a limited interest and importance for us till we attempt to integrate them with a further experimental observation, which is that self-regulation of the voice does not hold good for all people—but only for 'normal' and neurotic. The insane, and in particular general paretics, will, during the noise, raise their voices hardly at all and at most in lesser degree (Hermann & Fónagy, 1958). These, however, are just exactly the people whose unconscious contents we would expect to appear without hindrance because of diminishment of the critical capacity of the ego and of the power of inhibition. They can pursue their hallucinatory ventures, like the normal person dreaming, without having to attend to or deal with competition from the sensorial end of their psychic apparatus. With them simple repression of thought is ineffective and the repressed has little difficulty in reappearing (Hollós and Ferenczi, 1925).

These parallel research facts and clinical experiences require explanation. On the one hand we have as a normal result of scotomization an intensification of impulses, with overproduction that may possibly go too far. On the other hand in cases where the critical and

repressive capacity of the ego is damaged we note at most only a slight intensification of the impulses. In the case of general paresis we can guess the explanation. Because of disease the central nervous system fails to supply the reflex of self-regulation, just as the pupil fails to react to light. Clearly we cannot apply this explanation, derived as it is from the study of reflex-like self-regulation directed by the central nervous system, to other cases which are concerned with repression and other forms of non-reflex self-regulation; but we may surmise that in cases of unsuccessful repression and dangerous over-production, that it is the ego and its vigour which has suffered injury so that it can no longer properly carry out its own self-regulatory functions. The principal cause of this lack of vigour in the ego is the renunciation by the ego of its self-regulating activity, and its surrender to grown-ups who want to regulate it. The deficiently functioning ego of a neurotic child is one which has renounced self-regulation out of subordination to another.

Ego-subordination to another is well illustrated in the reactions of shame, which interestingly enough is present in most of the clinical situations mentioned above (blushing, enuresis). As I have stated elsewhere, shame is an elaborated form of anxiety and, unlike those forms of anxiety in which grasping and clinging to objects is active, in shame such relationships are forbidden. In shame the subject is not permitted to look at the shame-bringer, nor to grasp at him even with the eyes; the feet sink in the ground and the arms and head droop. The ego, humiliated by the shame-bringer, falls into servitude and becomes contingent on him. This is an entirely different state of affairs from that of ordinary anxiety where the ego strives for a dual-union link with the other, stronger ego, just as the infant cuddles his mother (Hermann, 1941).

To sum up: in repression the strength of the repressed impulse intensifies and this enhances the watchfulness of the repressor. This process however requires an ego capable of self-regulation. Outer influences and provocations tend

to create a subordinated ego no longer capable of self-regulation and similar in character to the shame-feeling ego. The subordinated ego easily sets free what should be controlled and

repressed. Probably true repression is often the result of intense repression and intensified impulse—which of course can reappear thereafter in new guises.

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THE MODIFIED WORD-LEARNING TEST: A CROSS-VALIDATION STUDY

By D. WALTON,* J. GRAHAM WHITE, D. A. BLACK AND A. J. YOUNG

A recent study (Walton & Black, 1957; Walton, 1958*a*) discussed the validity of a new learning test for diagnosing organic brain damage. Important differences in the speed of learning previously unknown verbal material were demonstrated between a combined group of 223 co-operative non-brain-damaged psychiatric patients and normal controls, and a group of 46 brain-damaged patients. Two main conclusions emerged:

(1) The test appeared capable of differentiating organics with *general cortical damage* from functionals and normals, with a negligible degree of misclassification.

(2) Although intelligence, vocabulary level and age exerted some influence on the test score, this was not sufficient to produce 'organic' scores independently of the presence of organicity.

In an effort to cross-validate these findings, a further 304 patients and normal controls have been tested.

METHOD

(1) *Administration and scoring*

The administration of the test was the same as reported in the original publication (Walton & Black, 1957). In the light of greater experience with the test, however, and in view of difficulties in grasping the scoring system mentioned by several correspondents, the time has been thought opportune for a revision of the scoring system.

Two considerations have been taken into account in this revision:

(a) Traditionally in test procedures low scores are 'poor' whilst high scores are 'good'.

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Yet the scoring in this test has been the reverse of such tradition.

(b) It is clearly more equitable that the scoring system should reflect the number of trials required for learning than that it should take into account the position in the list of the sixth word learned.

The result of these considerations has been to allot a single score for any successful performance within one presentation. And now that the limits of test performance are more clearly known and a workable cut-off point established at 26 on the old scoring method (end of the fifth presentation) it has also been decided to allot a maximum score of 10 for any successful performance on the first presentation, and to subtract 1 from the total score for each additional presentation required before success is achieved. Thus success on the second presentation of the ten words would score 9, on the third presentation a score of 8, on the fourth 7, and so on. In subsequent tables scores according to both the old and the revised systems are given. In order to avoid confusing the reader, only the old scoring system is used in the text.

(2) *Subjects used*

There were 304 subjects in all, composed of 83 normals, 66 neurotics (including 16 depressives), 32 psychotics (11 depressives), 45 mental defectives and E.S.N.'s and 78 organics.

The normal sample was screened for evidence of neuroticism and cerebral trauma, while testing of patients was restricted in all cases to those of unequivocal diagnoses.

The 78 organics included: 42 cases of senile dementia, pre-senile dementia and cerebral arteriosclerosis, 12 cases of general paralysis, 2 Korsakov psychotics, 1 chronic encephalitic, 4 post-encephalitics, 2 cases of post-

encephalitic Parkinsonism, 4 alcoholic psychotics, 1 cerebral atrophy secondary to meningitis, 1 hypertensive encephalopathy, 1 post-traumatic psychosis following head injury, 1 organic deterioration following chronic use of phenobarbitone, 1 organic confusional state, 1 organic condition with left-sided hemiplegia and Parkinsonism, 1 left-sided cerebral damage associated with a temporal epileptogenic focus, 1 case of frontoparietal damage, 1 closed head injury, 1 case of left hemisphere damage, 1 cerebral trauma involving base of skull.

(3) Reliability

In order to establish the reliability of the original findings it was necessary for several testers to take part in the study and for this testing to be undertaken in a large variety of institutional settings.

The greater part of the testing was completed by the authors working in six different institutions. Five other clinical psychologists contributed to the total by testing a number of non-brain-damaged and brain-damaged psychiatric patients of unequivocal diagnosis.

this cut-off point none of the 181 normals, neurotics or psychotics was misclassified. 66 (85%) of the 78 organics scored at or above this point. Thus, of the 259 patients tested in these four groups, 247 or 95% were correctly identified.

The original validation data for these same groups, combined with the cross-validation results of the normal, neurotic, psychotic and organic patients, represents a normal and patient population of 517 subjects. 502 of these subjects were correctly identified as either brain-damaged or as not brain-damaged.

The first of the two major conclusions of the validation study has been confirmed. The test appears capable of differentiating non-brain-damaged normal controls, neurotics and psychotics from adult organic cases suffering from general brain damage.

Table 2 shows the number of correct diagnoses based on M.W.-L.T. scores for each of the different organic groups. 85% of the senile dementia group were correctly identified. This satisfactory degree of differentiation has been supported recently in an independent study

Table 1. *Distribution of scores for the normal controls, functionals and organic subjects*

M.L.T.		Normals	Neurotics	Psychotics	M.D.'s and E.S.N.'s	Organics
Old score	New score					
1-5	10	49	28	10	9	4
6-10	9	22	24	6	6	1
11-15	8	8	9	8	3	3
16-20	7	2	4	4	4	1
21-25	6	2	1	4	4	3
26-30	5	—	—	—	1	5
31-35	4	—	—	—	2	13
36-40	3	—	—	—	8	11
Over 40	3	—	—	—	8	37
Totals		83	66	32	45	78

RESULTS

Table 1 shows the distribution of the scores for the normal, the non-brain-damaged psychiatric patients and for the brain-damaged group. The optimum cut-off point is 26. With

of the predictive validity of the M.W.-L.T. in psychiatric patients over 65 (Walton, 1958*b, c*). Few misclassifications were found in any of the organic groups with the exception of the post-encephalitics and post-encephalitic Parkinsonisms. Of the six cases

in the latter two groups, however, four were misclassified.

In order to elucidate this high percentage of misclassification electro-encephalographic examination was requested and completed for these six patients and the one case of chronic encephalitis. Seven cases of general paralysis suffering from unequivocal and generalized cerebral damage were also examined and their E.E.G. results used as controls. The problem was to decide whether the misclassifications with the M.W.-L.T. had occurred because the encephalitic patients were no longer brain-damaged (i.e. had fully recovered from their encephalitis) in contrast to the permanently brain-damaged G.P.I.'s, whether the damage was of limited locus and therefore more liable to produce test misclassifications, or whether the patients were in fact suffering from general cortical damage and had been misclassified on the M.W.-L.T.

close to the brain-damaged cut-off point (24) as to be suggestive of damage. In contrast, four of the E.E.G. records of these patients were normal. It does not therefore appear an appropriate technique by which to evaluate whether the seven other long-standing encephalitics were brain-damaged or not. The encephalitics with normal E.E.G.'s may therefore either have recovered from their encephalitis and not now be brain-damaged or be permanently brain-damaged with a correspondingly bad prognosis because the cerebral neurones have ceased to function. There appears no way of determining which would apply in the present instance. Turton (1958) has raised similar objections recently to the validity of the E.E.G. as a diagnostic aid to dementia, whilst the Williams paradox of a brain-damaged person with a normal E.E.G. is well known (Gibbs, 1955): '... a normal electroencephalogram can be bad news for the patient. There is the other

Table 2. *Number of correct diagnoses for each type of brain damage*

Type of brain damage	N	Correct diagnosis	Percentage misclassification
1. Senile dementia, pre-senile dementia, cerebral atherosclerosis	42	36	15
2. General paralysis	12	11	9
3. Korsakov psychosis	2	2	—
4. Chronic encephalopathy	1	1	—
5. Hypertensive encephalopathy	1	1	—
6. Cerebral atrophy secondary to meningitis	1	1	—
7. Organic confusional state	1	1	—
8. Organic deterioration following chronic administration of phenobarbitone	1	1	—
9. Post-encephalitis	4	2	50
10. Post-encephalitic Parkinsonism	2	0	100
11. Alcoholic psychosis	4	4	—
12. Localized cerebral lesions	7	6	15
	78	66	15

The results (Table 3) suggest that the E.E.G. may be of limited value as a criterion against which to evaluate the validity of psychological tests when *long-standing* general cortical damage is suspected. Of the seven G.P.I.'s, six returned brain-damaged scores on the M.W.-L.T., while the seventh score was so

side, that an abnormal electroencephalogram can be good news for the patient. It means that there are neurones dysfunctioning, and there is the possibility that they will begin to function normally.'

It seems more appropriate to consider the validity of the M.W.-L.T., not against doubt-

Table 3. *Relationship of E.E.G., M.W.-L.T. score and diagnosis*

Patient	Diagnosis	E.E.G.	M.W.-L.T. score	
			Old	New
1	Chronic encephalitis	Apart from slight excess of slow activity the record is normal	40+	3
2	Post-encephalitis	Within normal limits	50	1
3	Post-encephalitic Parkinsonism	Record probably normal	32	4
4	Post-encephalitis	Slight excess of slow activity, mostly from temporal regions. Little response on hyper-ventilation or on photic stimulation	1	10
5	Post-encephalitis	Minor abnormalities, not of specifically diagnostic value	1	10
6	Post-encephalitic Parkinsonism	Abnormal record consistent with organic pathology	13	8
7	Post-encephalitic Parkinsonism	A normal record	12	8
8	General paralysis	An abnormal low voltage record. Significance obscure	33	4
9	General paralysis	A normal record	40+	< 3
10	General paralysis	Within normal limits	40+	< 3
11	General paralysis	An abnormal record consistent with cortical pathology	24	6
12	General paralysis	An abnormal record consistent with cortical pathology	40+	< 3
13	General paralysis	A normal record	40+	< 3
14	General paralysis	A low voltage record, probably within normal limits	40+	< 3

ful criteria of organicity such as are provided by the post-encephalitics in the present sample, but against the predictive validity of the scale in the clinically doubtful case of brain damage. If the scale is shown to be valid in that case, further retrospective inferences might be made as to the real possibility of test misclassification or diagnostic error in the post-encephalitic group.

The M.W.-L.T. was administered to 48 senile psychotics in 1955, though the test results were not examined in relation to diagnosis until a two-year follow-up had been completed. Eleven of the 13 changes in diagnosis within that two-year period were correctly predicted. Similarly 45 out of the 48 cases were correctly identified when the 1957 diagnosis was compared with the M.W.-L.T. completed in 1955 (Walton, 1958*b, c*). Similarly the predictive value of the test in the clinically doubtful case of brain damage has

also been demonstrated (Walton & Black, 1959). Fifty-seven cases were referred for psychological examination to determine presence or absence of brain damage. The M.W.-L.T. results were then compared either with the psychiatric diagnosis on discharge from hospital, or with the final psychiatric decision as to diagnosis. All the 28 functional patients were correctly identified, while 19 of the 24 organics were diagnosed correctly. Five patients diagnosed subsequently as epileptics performed like the normal and functional patients.

The results of the validation and cross-validation studies, together with the two predictive studies, raise the question whether the misclassified encephalitics were in fact brain-damaged at the time of the test. This is not to imply of course that cerebral interference did not at one time take place.

The general conclusion appears to be that

when brain damage is of a general order patients exhibit a much greater learning difficulty than do normal controls and non-brain-damaged psychiatric patients. The size of these differences appears to be of both diagnostic and predictive importance.

Special mention should be made of the performance of 27 neurotic and psychotic depressives. Depressives often present a major problem in differential diagnosis, and a separate set of scores for this group ought always to be provided. Table 4 shows these results. No score greater than 25 was obtained, though the results of both the neurotic and psychotic depressives were generally higher than those of the other non-brain-damaged psychiatric patients.

differentiation was afforded for patients with I.Q.'s of 60 or above. 77% were correctly identified. Patients of borderline imbecile or of imbecile intelligence found, as a group, the greatest difficulty in learning to the test's required criterion. Only 42% of the defectives with I.Q.'s in the 50's were able to learn, while none of the five imbeciles was able to do so. This is not a surprising result, since organic aetiology can either be demonstrated or presumed in imbecility (Penrose, 1949; Crome, 1954). The five imbeciles included in this sample may not therefore be true misclassifications.

Tables 6, 7 and 8 present evidence as to the validity of the second major conclusion from the original study: 'Although intelligence,

Table 4. *M.W.-L.T. scores of 27 neurotic and psychotic depressives*

New Score	10	9	8	7	6	Mean
Old Score	1-5	6-10	11-15	16-20	21-25	
16 neurotic depressives	4	5	4	2	1	10.1
11 psychotic depressives	1	3	1	3	3	14.9

Table 5. *Performance of 46 clinically diagnosed mental defectives on M.W.-L.T. in terms of level of intelligence*

I.Q. range	N	Non-organic scores	Percentage correct
90-99	2	2	100
80-89	2	2	100
70-79	9	7	77
60-69	13	9	69
50-59	14	6	42
40-49	4	0	0
30-39	1	0	0

The earlier encouraging results with the M.D. sample were only partly confirmed. Although 26 of the 45 clinically diagnosed M.D. patients did learn, 19 were misclassified. Within this group a decrease in I.Q. was paralleled by an increase in M.W.-L.T. score. The Spearman Rank Correlation Coefficient for this comparison was +0.51 (significant at $P > 0.001$ level). Table 5 shows the percentage of correct diagnoses at each I.Q. level for these 45 patients. A satisfactory degree of

vocabulary level and age exert some influence on the test score this is not sufficient to produce "organic" scores independently of the presence of organicity.'

(1) *The effects of age on obtained scores*

Table 6 shows that age did not exert so great an influence as to produce misclassifications in the 143 normal and neurotic subjects of the present sample. The results are consistent with the earlier findings. It will be

remembered that 182 normals and neurotic patients between the ages of 17 and 77 were tested originally. No misclassifications occurred in this sample. The highest score obtained was 25. Thus, of the 325 normals and neurotics tested in the two studies and covering the age range of 6 to 77 none was misclassified, i.e. none scored more than 25.

Examination of the distribution of scores over the different age ranges shows that there is an apparent increase in scores below year 14 and that the same applies after 40 years of age. Between these two extremes there appears an optimum period for learning.

The Spearman Rank Order Correlation Coefficient (R_s) between age and M.L.T. scores was -0.07 (not significant).

recorded, though 17 cases were included with I.Q.'s of 79 and below. The two studies therefore represent a normal and neurotic population of 200, all of whom were 30 years of age or less. The range of I.Q.'s for this combined sample was 79 and below to 120+. No subjects were misclassified with a cut-off point of 25.

The previously noted tendency for an increase in scores to accompany a decrease in I.Q. was confirmed. The Spearman Rank Order Correlation Coefficient of $+0.36$ was significant beyond the 0.001 level.

The important conclusion appears to be that although a highly significant correlation was obtained between intelligence and test score, this influence was not strong enough

Table 6. *Effect of age on the scores of 143 normal and neurotic children and adults between the ages of 6 and 77*

N	Age range	M.L.T. scores					
		Old New	1-5 10	6-10 9	11-15 8	16-20 7	21-25 6
7	6-7		1	3	1	1	1
21	8-10		8	7	4	1	1
22	11-13		12	7	3	—	—
14	14-16		8	6	—	—	—
28	17-19		24	3	—	1	—
15	20-29		12	3	—	—	—
4	30-39		2	1	—	1	—
13	40-49		4	7	2	—	—
10	50-59		2	3	3	1	1
9	60-77		2	3	3	1	—
143			75	43	16	6	3

(2) *The effects of intelligence on obtained scores*

Table 7 shows the effect of I.Q. on the scores of 109 normal and neurotic children and adults who were matched for age. No misclassifications occurred at any I.Q. level. This result might be evaluated against the effect of I.Q. on the scores of 91 normals and neurotics in the validation sample. Although there was a tendency for the scores to increase with a decrease in I.Q. no score greater than 19 was

to produce test misclassifications independently of the presence of brain damage.

(3) *The effects of vocabulary on obtained scores*

Table 8 shows the effects of vocabulary on the scores of 110 normal and neurotic children and adults between the ages of 6 and 30. No subject scored greater than 25, though the vocabulary level in some cases was as low as from 5 to 9 words. Although the correlation between vocabulary and obtained scores

Table 7. *Effect of I.Q. on the scores of 109 normal and neurotic children and adults between the ages of 6 and 30*

		M.L.T. scores					
		Old	1-5	6-10	11-15	16-20	21-25
N	I.Q.	New	10	9	8	7	6
10	120+		7	1	2	—	—
25	110-119		17	6	1	1	—
29	100-109		20	6	2	—	1
20	90- 99		13	4	1	1	1
19	80- 89		7	9	3	—	—
6	79 and below		1	4	—	1	—
109			65	30	9	3	2

Table 8. *Effect of vocabulary on the scores of 110 normal and neurotic children and adults between the ages of 6 and 30*

		M.L.T. scores					
	Vocabu- lary	Old New	1-5 10	6-10 9	11-15 8	16-20 7	21-25 6
6	30-34		5	—	1	—	—
6	26-29		5	1	—	—	—
38	20-25		31	7	—	—	—
25	15-19		16	6	2	1	—
23	10-14		7	8	5	2	1
12	5- 9		2	8	1	—	1
110			66	30	9	3	2

was very high (+0.63, significant > 0.001 level), this influence was again not strong enough to produce misclassifications in the absence of brain damage.

The second major conclusion of the validation study has therefore been confirmed at least tentatively, although it is clearly necessary to collect more results from normal and neurotic subjects over 40 years of age, who are also of low intelligence and vocabulary.

SUMMARY

In a recent study the validity of a new learning test for diagnosing organic brain damage was reported. Two main conclusions emerged.

(1) The test appeared capable of differentiating organics with *general cortical damage* from functionals and normals, with a negligible degree of misclassification.

(2) Although intelligence, vocabulary level and age exerted some influence on the test score, this was not sufficient to produce 'organic' scores independently of the presence of organicity.

In an effort to cross-validate these findings, 83 normals, 66 neurotics, 32 psychotics, 45 M.D.'s and E.S.N.'s and 78 organics were tested. This testing was undertaken by nine clinical psychologists working in ten different institutional settings.

The two major conclusions of the validation study were confirmed. A higher percentage of misclassification was, however, found in the mentally defective group. A partial explanation may be that the misclassified imbeciles were brain-damaged, an interpretation in accordance with current opinion (Penrose, 1949; Crome, 1954). Fewer mis

classifications were found in subjects of high-grade defective intelligence.

ACKNOWLEDGEMENTS

We would like to thank the following clinical psychologists who kindly tested patients for inclusion in the present study: Mr K. S. Marszalek (St Luke's Hospital, Middles-

brough), Mr P. Ley (Netherne Hospital), Miss F. Levinson (Alder Hey Children's Hospital), Mr K. J. Batchelor (Fulbourn Hospital, Cambridge), Dr V. Hamilton (Holloway Sanatorium, Virginia Water). We would also like to thank Dr R. B. Taylor (Rainhill Hospital) for carrying out E.E.G. examinations on selected cases.

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WALTON'S MODIFIED WORD-LEARNING TEST WITH CHILDREN

By JOHN GRAHAM WHITE*

Walton & Black (1957) have recently described the use of a word-learning task as a test of brain damage with adults. To establish the lower limits of usefulness of this technique two studies were undertaken with children as subjects. It was thought likely that organic brain pathology would probably have a different meaning in the case of the immature brain from that obtaining in the case of adults; and that, therefore, a diagnostic instrument found useful with the latter might have little relevance with the former. It was considered for example that, in the case of the growing brain, age, intelligence, and vocabulary level might have different effects on learning test scores from those found in the case of the mature brain. However, it was also considered possible that a group of brain-damaged children, who have particular difficulty in controlling their attention in a learning situation, would experience more trouble with this learning task than would non-brain-damaged children of like age, intelligence, and vocabulary level.

FIRST STUDY

Effects of age, intelligence and vocabulary

To examine the first proposition, that age, intelligence and vocabulary would affect differentially the learning test scores of children and of adults, 58 children, ages 6 to 16, 40 of them normal and 18 psychiatric referrals, were tested. None of them was suffering from any known brain pathology at the time of examination. Eight defective children were also tested, one of whom was an epileptic and

has been omitted from the distribution of scores showing the effect of intelligence. None of these was included in the distributions for age and vocabulary.

The task for the children, as for Walton's adults, was to learn the meanings of six out of ten consecutive words from the Terman-Merrill, Form L, Vocabulary Scale previously failed by the child. High scores on the test indicate that a large number of presentations of the ten words was necessary before the meanings of six were learned, low scores represent a small number of presentations. Thus, a score of 36-40 is equivalent to eight presentations, a score of 21-25 to five presentations and a score of 1-5 to only one presentation. For a fuller description of the method of scoring the reader is referred to Walton & Black's original report (1957) and to the more recent cross-validation study by Walton *et al.* (1959).

The results of this first study are presented in Tables 1, 2 and 3. It is clear from the tables that most of these children do not score above 10 (71, 78 and 71 %, respectively, for the three tables). Furthermore, not one of the 58 children not suspected of brain damage obtained a score greater than 24. Rank order correlations of age and intelligence with learning test score are zero. Vocabulary level, however, exerts some influence on the learning score, as is shown by the correlation of 0.48, which is fully significant. Of the eight defective children tested seven obtained scores of less than 18 and five of these had scores of less than 10. The eighth child was unable to learn. For the probability of seven of the eight being able to learn within the range established for normals a Chi-square value of 3.12 for one degree of freedom is significant at $< 0.05 > 0.025$ level of confidence.

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Table 1. *Effect of age on the Modified Word-Learning Test scores of 58 children, aged 6 to 16 years, suffering from no known brain pathology. (M.D. children omitted)*

N	Age	M.W.-L.T. scores				
		1-5	6-10	11-15	16-20	21-25
7	6-7	1	3	1	1	1
21	8-10	8	7	4	1	1
22	11-13	12	7	3	—	—
8	14-16	5	3	—	—	—
58		26	20	8	2	2

Spearman rho = 0.030.

Table 2. *Effect of intelligence on Modified Word-Learning Test scores of 65 children, aged 6 to 16 years, suffering from no known brain pathology*

N		M.W.-L.T. scores					
		1-5	6-10	11-15	16-20	21-25	Over 40
5	120 and above	4	—	1	—	—	—
14	110-119	6	6	1	—	—	—
11	100-109	6	2	2	1	—	—
10	90- 99	4	3	1	—	1	—
18	80- 89	6	9	3	1	1	—
3	70- 79	—	2	—	—	—	—
4	60- 69	3	—	—	1	—	—
65		29	22	8	3	2	1

Spearman rho = 0.003.

Table 3. *Effect of vocabulary level on Modified Word-Learning Test scores of 58 children, aged 6 to 16 years, suffering from no known brain pathology (M.D. children omitted)*

N	Vocabulary	M.W.-L.T. scores				
		1-5	6-10	11-15	16-20	21-25
2	25-29	1	1	—	—	—
7	20-24	6	1	—	—	—
16	15-19	10	4	2	—	—
21	10-14	7	6	5	2	1
12	5- 9	2	8	1	—	1
58		26	20	8	2	2

Spearman rho = 0.481.

$t = 4.09$ for d.f. = 56, significant at beyond $p = 0.0005$ level for the one-tailed test.

SECOND STUDY

Comparison of the learning of brain-damaged and normal children

Cerebral-palsied children, who constituted the bulk of the experimental group used in this study, present many problems for effective teaching that must be attributed, directly or indirectly, to the condition of the damaged brain. It is true that they suffer from lesions whose major impact is on the motor areas, and one finds that they experience particular difficulty with perceptual organization, while their verbal ability is often unimpaired. However, one sees other effects, both cognitive and affective, that are characteristic of damage located in other parts of the brain and not only in the parieto-occipital areas. These effects have been well summarized by Strauss & Lehtinen (1947) and Strauss & Kephart (1955) in their two volumes on the education of the brain-injured child. Thus, cerebral palsied children tend to present as a group with severe learning difficulties on account of the often gross emotional disinhibition from which they suffer, in common with children who have sustained injury to other areas of the brain, as, for example, those who have recovered from tuberculous meningitis. Their over-responsiveness to minimal stimulation makes it difficult and sometimes impossible for them to sustain attention on cognitive tasks for more than a few moments at a time.

For these reasons, and also because it was necessary to have a brain-damaged group in which the great majority of cases had suffered

injury to both hemispheres, it was decided to compare the performance on Walton's learning task of such a group with that of another group, matched for age, intelligence, and vocabulary level, composed of children free from brain damage. It was proposed that the performance of the brain-damaged group would be inferior to that of the control children.

Out of a total of 30 brain-injured children an experimental group was formed, consisting of 17 children, eight boys and nine girls, 13 suffering from spastic quadriplegia or diplegia (two cases of the latter), three with a history of meningitis (one tuberculous, one pneumococcal, and one of uncertain diagnosis), and one case of porencephaly in which the medial part of the left occipital lobe had been removed, whose disturbed behaviour made it difficult for the child to be taught in school. A minimum of three years had elapsed between discharge from hospital and testing with the modified word-learning test. In most cases the interval was, of course, much longer. Each of these children was matched for age, intelligence and vocabulary with another child who was free from any suspicion of brain damage.

The matching proved highly satisfactory (see Table 4), no difference between the groups in the three controlled variables diverging significantly from zero. However, when their word-learning scores were compared by Wilcoxon's Matched Pairs Test the two groups proved to be indistinguishable ($T = 48$ for $N = 16$). These brain-damaged children could learn as well as their non-brain-damaged controls.

Table 4

	Brain-damaged children N = 17 Sex: 8 boys, 9 girls		Non-brain-damaged children N = 17 Sex: 10 boys, 7 girls	
	Mean	S.D.	Mean	S.D.
Age (years)	11.09	2.56	11.06	2.54
Intelligence (I.Q.)	84.82	10.69	86.65	10.34
Vocabulary (Form L)	11.82	3.47	11.12	3.26

DISCUSSION

These results require only brief comment.

First Study. While age and intelligence have no effect on this learning task in the case of the normal or neurotic child, the task became impossible for children below the age of six years, probably because the difficulty gradient of the Terman-Merrill, Form L, Vocabulary is too steep at these early ages. This is consistent with the relationship shown to exist between vocabulary level and learning test score.

Second Study. Why did brain damage, as defined for the purposes of this study, have so little effect on learning? While there was no doubt in any case about the fact of injury, and its continuing intracranial effects were still evident in the E.E.G. records of all those children submitted for examination, the spastics as a group could learn. This was presumably because those areas of the brain most affected do not mediate this kind of learning, despite the fact that the brain injury is often attended by considerable attention defects which interfere with other forms of learning.

The fact, however, that four of the nine children who could not learn happened to have histories of some form of meningitis is interesting. It suggests that there are certain kinds of brain damage in children whose effects on learning may be comparable with those associated with the cortical degeneration found in older people and that these effects may be prolonged, if not permanent. This possibility will be investigated further in a follow-up study of children who have been treated for tuberculous meningitis during the last ten years.

CONCLUSIONS

(1) Age and intelligence were shown to have no effect on the learning test scores of children. Vocabulary level, on the other hand, was shown to exert a significant influence on these scores.

(2) The fact that no normal child, nor any child referred on account of a behaviour disorder to a psychiatrist, obtained a score above

25 supports Walton's choice of that score as a suitable cut-off point.

(3) Brain-damaged children suffering predominantly from spastic quadriplegia and attendant learning difficulties can learn as well on Walton's test as normal children, although it should, perhaps, be noted that the only children who failed to learn, or who obtained abnormally high scores, were eight brain-damaged children and one mentally deficient child.

SUMMARY

An attempt was made to explore the lower limits of usefulness of Walton's Modified Word-Learning Test. To this end 96 children between the ages of 6 and 16, of known intelligence and vocabulary level, were tested, 66 of them being either normal, psychiatric referrals but not brain-damaged, or feeble-minded, and 30 of them brain-damaged. Most of the latter were cerebral-palsied children, either spastic quadriplegias or diplegias; but four old cases of meningitis and one of porencephaly were also included.

It was found (1) that, although it proved impossible to draw regression lines for either age or intelligence and M.W.-L.T. score, in the case of vocabulary a substantial and significant correlation appeared between low vocabulary level and high M.W.-L.T. scores; (2) that, while none of the non-brain-damaged children scored above an empirical cut-off point of 25 on Walton's learning task, neither did many of the brain-damaged group.

It was concluded (1) that age and intelligence have no effect on the learning scores of normal and neurotic children, thus supporting Walton's original finding with adults; (2) that vocabulary level exerts a significant influence on their scores, but not so great as to produce 'false positive' scores; and (3) that spastics as a group learn as well as normal children; but that (4) somewhat different results might be expected from a group of children who had suffered the effects of tuberculous, pneumococcal or meningococcal meningitis, since these children experienced great difficulty in learning. It was noted that the only children

who failed to learn, or who obtained abnormally high learning test scores, were all but one brain-damaged.

ACKNOWLEDGEMENTS

I should like to record my gratitude to Dr A. M. Brown, Senior School Medical Officer, Liverpool, for his help in the selection of the brain-damaged children, to Dr R. J. Derham,

of the C.P. Centre, Alder Hey Children's Hospital, for evaluating the anatomical evidence in the case of the spastic children, to Dr R. R. Hughes, Consultant Physician of the Royal Southern Hospital, for carrying out E.E.G. examinations of seven of these children, and to the Head Teachers of the special schools for their ready co-operation in this study.

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FAMILY RELATIONS AS EXPERIENCED BY PSYCHOLOGICALLY DISTURBED CHILDREN*

BY EVA BENE*

Most studies of children's emotional attitudes towards their families have been directed towards proving or disproving the Oedipal hypothesis. Various methods have been employed to find out what these attitudes are. Some studies were based on projective techniques and involved the interpretation and organization of masses of phantasy material with all the attendant hazards. Others used rather direct methods, such as asking the children which of their parents they liked best, or asking in doll-play whether the doll preferred its mummy or daddy. The results obtained varied with the methods used and with the age of the sample. Ammons & Ammons (1949) found that pre-school children prefer the parent of the same sex. Newell (1932) and Friedman (1952) showed that older children prefer the parent of the opposite sex. Kuppuswamy (1949) found that both boys and girls prefer their mothers. So did Simpson (1935), who claims that there is a specially marked preference for the mother on the part of both sexes by the age of ten. He also found that the girls' preference for the mother was more pronounced in responses to projective than to direct techniques.

The aim of the present study was to find answers to the following questions:

(1) How do the feelings psychologically disturbed children have towards their parents compare with the feelings they think their parents have towards them?

(2) How do the relationships the children have with their fathers compare with those they have with their mothers?

(3) How do the relationships they have with their older siblings compare with those they have with their younger siblings?

The investigation of these relationships was

* Manuscript received 11 June 1958.

not restricted to feelings whose existence children could openly acknowledge. It also included their more or less private feelings, which they would find difficult to discuss, although it did not extend to feelings of whose existence they were not aware.

The technique employed in the present investigation, called 'the Family Relations Test', has been described elsewhere by Anthony & Bene (1957). Here it suffices to say that it is a semi-projective technique which gives objective scores, and which seems to be reasonably valid and reliable. It enables the child to create what could be called his own 'family sociogram', by placing himself at the centre of his emotional relationships in his home and giving him means by which he can assess the feelings in which he is involved. The child is not expected to analyse or verbalize his many complex emotions, or to express preference in terms of over-simplified generalizations. The test situation is basically a play situation. The child is seated at a table, in front of him are figures representing the members of his family, and plays a sort of 'guess who' game in which he decides for each item whether its content fits anyone in his family, and if so, whom. The emotional attitudes with which the test is concerned include mild feelings of like and dislike, stronger feelings of love and hate, sex and aggression in the widest sense of these words, and jealousy reactions. The meaning of these attitude areas is defined by the meaning of the items used to test them. Since the emotional life of a child depends not only on the feelings he has for other people but also on the feelings he thinks other people have for him, some items express feelings which emanate from the child and are directed towards other person's while other items serve to reflect the child's,

perception of the emotions the members of his family have towards himself. The test is constructed in such a way that the child is under no pressure to admit to feelings which do not exist in his situation, or which do exist, but which he cannot acknowledge. When a single record is examined, the number and pattern of the omitted items indicate the kind and strength of his defences in operation. When, however, a statistical analysis is made of many records it is very difficult to distinguish the items that were discarded for direct or genuine reasons from those that were discarded for indirect reasons of defence, and this is why the children's unconscious feelings were not taken into account in this study. The results however do include the kind of feelings which children keep quietly to themselves because they do not know how to put them into words, or because they would feel too anxious and guilty if they were to do so.

The sample

The children studied were outpatients at the children's department of the Maudsley Hospital. They had been referred for the reasons which are usual with 'child guidance' cases. The majority came from working-class or lower middle-class homes in the south-east of London. Their I.Q.s ranged from 67 to 144 with a mean I.Q. of 98, and they were from 7 to 15 years old, with a mean of 11 years. Children who came from broken homes were excluded from the investigation.

Two samples were used in the study, each consisting of current outpatients at a given period of time. The second sample was used to see whether the results obtained from the first could be replicated. The first sample consisted of 30 boys and 26 girls, and the second sample of 57 boys and 21 girls. More boys than girls were referred for psychiatric treatment.

RESULTS

(1) Reciprocity between feelings for and from parents

The present study aimed at answering several questions. First it was asked what

the relationship might be between the feelings the children had for their parents, and the feelings they thought their parents had for them. To find the answer, the affectionate, sexual, and negative feelings the children expressed towards their fathers and mothers were correlated with the corresponding feelings experienced as coming from their fathers and mothers towards them. The reciprocity of hostile feelings was not examined because too many children had zero scores in this area. There were zero scores in the other areas as well; a number of children, for instance, scored zero regarding sexual feelings for and from the father, or negative feelings for and from the mother. Although these zero scores made the assessment of statistical significance difficult, the correlation coefficients which were obtained (Table 1) indicate that there was a good deal of reciprocity between the feelings the children had for their parents and the feelings they thought their parents had for them. This give and take of feeling was especially pronounced in relation to affectionate feelings.

The reciprocity between outgoing and incoming feelings made it possible to combine the corresponding areas for the sake of simpler comparisons.

(2) Comparison of the relationships the children had with their fathers and mothers

Children who are in their latency and puberty years are not a suitable sample for the demonstration of Oedipal feelings, since the sexual desires for the parent of the opposite sex, and the hostility for the parent of the same sex have usually been repressed by that time. Still, one might expect some indication of cross-sexual parental preferences, even at that age, and even with psychologically disturbed children. This expectation was more than fulfilled by the boys, but not at all by the girls, since the results (Table 2) showed that both boys and girls had more ambivalent relationships with their fathers and more positive relationships with their mothers. Mothers were given more items expressing affectionate

Table 1. *Correlations between attitudes children expressed towards their parents and attitudes they thought their parents had towards them*

	Boys			
	First sample N = 30		Second sample N = 57	
	Father	Mother	Father	Mother
	<i>r</i>	<i>r</i>	<i>r</i>	<i>r</i>
Affectionate	0.81	0.85	0.82	0.77
Sexual	0.86	0.64	0.67	0.57
Negative	0.50	0.44	0.50	0.54
To be significant at the 5 % level <i>r</i> must equal or exceed	0.36		0.26	

	GIRLS			
	First sample N = 26		Second sample N = 21	
	Father	Mother	Father	Mother
	<i>r</i>	<i>r</i>	<i>r</i>	<i>r</i>
Affectionate	0.78	0.73	0.77	0.87
Sexual	0.74	0.67	0.42	0.46
Negative	0.71	0.67	0.62	0.31
To be significant at the 5 % level <i>r</i> must equal or exceed	0.39		0.43	

Table 2. *Mean number of items given to parents expressing affectionate, sexual, negative and hostile attitudes towards them*

	Boys					
	First sample N = 30			Second sample N = 57		
	Father	Mother	Level of Signif.	Father	Mother	Level of Signif.
Affectionate	7.2	9.1	0.05	5.8	8.5	0.01
Sexual	2.5	5.9	0.01	1.8	4.2	0.01
Negative	4.8	2.5	0.01	4.4	1.9	0.01
Hostile	1.2	0.7	—	1.7	0.5	—

	GIRLS					
	First sample N = 26			Second sample N = 21		
	Father	Mother	Level of Signif.	Father	Mother	Level of Signif.
Affectionate	5.6	8.4	0.01	5.6	7.9	0.01
Sexual	3.4	6.9	0.01	2.5	4.3	0.05
Negative	4.3	2.0	0.01	2.6	1.7	—
Hostile	2.0	0.2	—	0.5	0.2	—

and sexual feelings and fathers were given more items expressing negative and hostile feelings by children of both sexes. The lack of difference between the pattern of the boys' and girls' scores was so remarkable that the investigation was repeated to see whether it would recur. In the case of the second sample the boys showed more and the girls showed less ambivalence towards their father but the basic picture of ambivalent feelings for father, and loving feelings for mother, remained the same.

Statistical comparisons of the data showed that the boys and the girls of both samples expressed significantly more affectionate and sexual feelings for their mothers than for their fathers. The boys of both samples, and the girls of the first sample, expressed significantly more negative feelings against their fathers than against their mothers. The girls of the second sample had also more negative feelings against their fathers than against their mothers, but the difference was not significant. Hostile feelings had not been expressed frequently enough for statistical comparisons.

The differences between the children's relationships with their fathers and with their mothers seem to have been larger with regard to the quality of feeling than with regard to its quantity. One might have thought that fathers would receive considerably fewer responses since they spend less time at home with the children and do not take such an active part in their lives. However, the results indicate that the children were only slightly less involved with their fathers than they were with their mothers, so that their fathers' absence does not seem to have detracted too much from his importance.

(3) Comparisons of the relationships the children had with their older and their younger siblings

In this part of the study the two samples were combined because the children of the separate samples did not have enough siblings to make all comparisons meaningful. Statistical significances were not computed, because of the varying numbers of brothers

and sisters between whom the children had distributed the items of the test.

The data showed that, as a rule, the children experienced the relationships with their siblings as consisting of a mixture of love and aggression. The older siblings expressed a predominance of loving over aggressive feelings towards their younger brothers and sisters and the younger siblings expressed a predominance of aggressive over loving feelings towards the older ones. Babies and toddlers formed a special group towards which both boys and girls expressed almost pure love and affection. The only other relationship expressed in terms of loving feelings was the one older sisters had with their younger sisters.

The rank order, in which older and younger siblings of the same and opposite sex were given items expressing loving feelings is shown in Table 3. Both boys and girls expressed most loving feelings towards their infant brothers and sisters, next came the older siblings of their own sex, next the younger siblings of their own sex, then the younger siblings of the opposite sex, and finally the older siblings of the opposite sex. Table 3 shows that the greatest number of aggressive items was given to the older siblings of the same sex and the smallest number was given by both boys and girls to the younger sisters and to the infants in the family.

In sibling relationships quality and quantity of feeling did not seem to be connected in any constant manner. Both boys and girls appear to have been very much involved with the older siblings of their own sex and with their infant sisters and brothers; but they expressed considerable aggression towards the former and practically none against the latter. The boys expressed little interest in their younger sisters and the girls not much in their older brothers; but, relatively speaking, the boys appear to have been quite fond of their little sisters whereas the girls seem to have rather disliked their older brothers. To be psychologically important and to be loved is not necessarily the same thing.

Table 3. *Rank order of loving and aggressive feelings expressed towards siblings*

Combined means of affectionate and sexual responses			
BOYS		GIRLS	
Baby	8.8	Baby	12.4
Older brother	6.2	Older sister	6.9
Younger brother	5.3	Younger sister	5.8
Younger sister	4.8	Younger brother	5.7
Older sister	3.9	Older brother	2.9
Combined means of negative and hostile responses			
Older brother	7.5	Older sister	6.5
Older sister	5.2	Younger brother	4.6
Younger brother	4.6	Older brother	3.7
Younger sister	2.8	Younger sister	1.8
Baby	1.7	Baby	1.1

DISCUSSION OF RESULTS

The results suggest that the children investigated here felt that there was considerable reciprocity between the feelings they had for their fathers and mothers and the feelings they thought their fathers and mothers had for them.

The present results confirm the findings of Simpson (1935) and Kuppaswamy (1949), according to which the parent that both boys and girls love most is the mother. The children who formed the sample of this investigation were more involved with their mothers than with anyone else in their families and the relationships they had with her were highly positive. Their dominant feeling towards their mother was affection and the next most important was sexualized love, the desire for physical proximity.

The only other relationship which the children experienced as overwhelmingly positive, and in which they were highly involved, was the one with their infant sisters and brothers. In this relationship the desire for physical contact was as strong as was affection, while negative and hostile feelings were minimal.

The girls appeared to have one other relationship in which love was predominant, the one with their younger sisters. However,

youngers sisters were far less important to them than were the infants.

It seems that all other family relationships of the children were characteristically ambivalent. They were much involved with their fathers, with much affection for him, but also with much negative feelings, and with less sexualized love than in the case of the mother, even by the girls. Hostility, on the other hand, was rather more pronounced.

It appears that persons of surprising importance to the children were the older siblings of their own sex. The children were as much involved with them as with their infant sisters and brothers, but in a rather ambivalent manner. There was much affection in this relationship, but not much desire for physical contact, and considerable negative feeling and hostility were present.

The boys' relationships with their younger sisters and brothers seem to have been of lesser importance. These relationships were ambivalent with some predominance of loving feelings of both kinds.

The siblings with whom the children were least involved were the older ones of the opposite sex. The balance of feeling in these relationships was more negative than in any other.

In conclusion it should be pointed out once more that these results refer to family

relationships as they were felt by the children, and not as they might have been assessed by another observer.

SUMMARY

This paper is concerned with the emotional relationships psychologically disturbed children believe to exist between themselves and the members of their families.

(1) A short description is given of the test which has been used in the investigation. This test, which is reasonably valid and reliable and which gives quantitative results, enables children to express their feelings about the members of their families without having to verbalize these feelings.

(2) A description is given of the sample, which consisted of children who were outpatients at a psychiatric hospital, and who were between 7 and 15 years old.

(3) The results of the investigation are discussed. They indicate that there is considerable reciprocity between the feelings these children had towards their fathers and mothers and the feelings they thought that their fathers and mothers had for them. They also seem to show that the majority of both boys and girls believed they had loving relationships with their mothers but ambivalent relationships with their fathers, loving relationships with the infants in their families, but ambivalent ones with the other siblings.

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PSYCHOTHERAPY BY RECIPROCAL INHIBITION: A REPLY TO DR GLOVER

By JOSEPH WOLPE

The note of agitation that pervades Dr Glover's critical notice (1959) on my book, *Psychotherapy by Reciprocal Inhibition* (1958), proclaims his awareness of the threat to the citadel of psychoanalysis whose foundations he knows lack the strength that the rigorous application of scientific method alone can give to a theory. Dr Glover uses every stratagem he can find in his effort to discredit the 'enemy'—not hesitating here and there to misreport the contents of the book in order to strengthen his case. For example, he gratuitously includes 'laying on of hands' among my methods; mis-states my definition of 'stimulus' (1958, p. 4); gives the number of cases indicated as having *simply* 'interpersonal anxiety' as 'not less than twenty-one' when the number is actually seven—having groundlessly assumed beforehand that this feature indicates an 'easy' case. There are a good many more of these distortions, each of them like the examples above giving an impression less favourable than an accurate account would give. But rather than discuss any further these or other matters of secondary importance such as Dr Glover's quibbles over words or his misconceptions about learning theory, I shall concentrate on the main issues.

The gist of Dr Glover's reasoning may be stated as follows. Psychoanalytic theory and practice have given us a wealth of knowledge of the processes that lead to neurosis and have shown us how neurosis should be treated. It is ridiculous for anybody to ignore this knowledge and to try to substitute a simple theory of neurosis based on principles of learning derived largely from laboratory studies. The clinician should not allow himself to be influenced by the good results claimed for the methods that have emerged from this simple theory. He should dismiss

these results and continue steadfastly to pin his faith on exploratory and interpretative methods of therapy, not allowing himself to be disheartened by his frequent therapeutic failures.

If the first part of this statement were true, if psychoanalytic 'knowledge' had really revealed the essence of the cause of neuroses and shown the way to the most effective methods of cure, it would indeed be absurd for either research on neurosis or its therapy not to make use of Freud. But theories acquire the status of knowledge only when they are sustained by observations in circumstances that provide proper testing. Very few psychoanalytic hypotheses can qualify as knowledge on this criterion. To a great extent the reason for this is that psychoanalytic statements are usually not testable, either because of lack of precise meaning or because 'they explain everything and predict nothing', as Eysenck has put it (1953). Dr Glover is fully aware of the poor scientific status of his creed. Some years ago, in a paper dealing with the appalling ignorance among psychoanalysts concerning scientific method, he wrote (Glover, 1952) '... A large proportion of current theorizing and clinical findings is little more than unchecked speculation.' Application of the scientific standards that he advocated in that paper to some of the psychoanalytic principles *he* regards as established would soon show him how many of these too are unchecked speculations even when Freud himself proposed them. One has only to study closely some of Freud's case histories to realize how easily his prepossessions blinded him to facts (Rachman and Wolpe, 1959).

Among the few psychoanalytic propositions that do lend themselves to testing there is one intimately connected with the crucial

hypothesis that repression has a central role in the production and maintenance of neurotic symptoms. This hypothesis generates the prediction that no case of recovery from neurotic symptoms can endure unless there has been uncovering and expression of repressed memories. Patients who have recovered from neurosis after non-analytic treatment are the obvious subject-matter for a test of this prediction. A recent survey of the literature (Wolpe, 1958) has shown that when the condition of neurotic patients is markedly ameliorated by methods that do not deal with repressions *relapse is rare*. For example, Hamilton, Varney & Wall (1941) following up their patients 5-15 years after treatment found no evidence of relapse in 66 out of 67 patients who had been apparently cured or much improved by non-psychoanalytic psychotherapy. Several other studies (e.g. Hamilton & Wall, 1941; Luff & Garrod, 1935; Stevenson, 1959) have produced similar observations, and nobody appears to have found any evidence to the contrary.

The above findings are unequivocally contrary to the expectations of the psychoanalysts and severely damaging to the repression theory of neurosis. It may be anticipated that psychoanalysts will disregard these findings as blissfully as they have in the past disregarded other criticisms of their theories and methods (e.g. Salter, 1952; Wohlgenuth, 1923). But to anybody not firmly indoctrinated it is clear that this evidence undoes the kingpin of psychoanalytic theory, and in so doing exposes a presumptuousness in Dr Glover's outcry against the sacrifice of 'painfully acquired psychological insights'. However painfully psychoanalytic 'insights' are acquired, validity is not thereby conferred on them.

In view of the manifest failure of psychoanalysts to provide a systematized body of *scientifically acceptable* knowledge, it is not unreasonable to explore other avenues. My own course started from the premise that all my behaviour, whether manifested to me as a movement or an idea, has a physiological

substrate. (It would be interesting to know whether Dr Glover denies this and believes that there are autonomous mental processes.) This premise naturally led to the study of what Dr Glover calls 'the naïve concepts of physiology'. The only known processes that can subserve lasting changes of response are physical growth, organic pathological states, and learning; and of these learning alone seems capable of explaining *neurotic* behaviour. Since synaptic changes are the apparent basis of learning it is proper to give them some consideration. This, contrary to Dr Glover's evident belief, does not require the mention of synapses whenever neurosis is being discussed any more than a discussion of organic chemistry requires constant reference to atomic structure. Recognizing the connexion between different aspects or 'levels' of a phenomenon does not imply 'reducing' one level to another. The interrelations of molar behavioural events can be studied at the molar level entirely, with merely a background realization that nerve impulses underlie all behaviour and that synapses change when learning occurs.

Modern learning theory has been built upon the study of simple reactions, and much knowledge has been accumulated regarding the factors influencing the learning and unlearning of such reactions. With this as a base I formulated hypotheses regarding the causal relations of the more complex phenomena of experimental neuroses. Certain of these hypotheses were supported by observation. The next step was to see whether similar hypotheses about human neuroses would be supported by observations of human neurotic behaviour. They were. This is normal scientific procedure. But because the outcome conflicts with his assumptions, Dr Glover raises a cry that the *difference* between humans and animals should have forbidden the examination of any hypothesis postulating that they have similar basic mechanisms of neurosis formation!

Among many other resemblances it was observed that both animal and human

neuroses can be overcome if the neurotic responses can be inhibited by evoking simultaneous responses incompatible with them. The neurotic manifestations are then progressively weakened, eventually disappearing—*lastingly*.^{*} Dr Glover would of course accept that recovery in the animals is a function of learning. But he attributes the human improvements to 'suggestion'. The reason for this, presumably, is that, in the psychoanalytic catechism, if repression-lifting and transference cure can be excluded the only remaining possible explanation for improvement is suggestion. Of course, some suggestion enters into all forms of psychotherapy, but suggestion therapy as such is rightly in low repute, since its effects are often trivial and usually brief. But suggestion can hardly be considered a probable explanation of the effects of reciprocal inhibition methods when 90% of the patients treated have achieved (after a mean of 31 interviews) marked clinical improvement, shown in 2-7 year follow-up reports to be almost invariably maintained. Learning provides an altogether acceptable explanation of such enduring changes. Is it unthinkable to psychoanalysts that learning, the principal modifier of the behaviour of mammals, has something to do with neurotic deviations? It is ironic that Freud, too, had reason to complain of being 'explained away' by the catchword 'suggestion'. He wrote (Freud, 1925, p. 244), 'Nobody knows and nobody cares what suggestion is, where it comes from or when it arises—it is enough that everything awkward in the region of psychology can be labelled "suggestion".'

The results obtained by reciprocal inhibition methods are at face value obviously far

superior to the 60% or less of good results obtained by analysts in ten times as many interviews (assuming an average of 300). Dr Glover tries very hard to explain away the unfavourable comparison. He raises a doubt (p. 74) whether I am to be believed. Then he implies that my cases are more simple and straightforward than those treated by psychoanalysts, and that it is their prerogative to achieve success with 'the more intractable and inaccessible cases'. This is at best a shot in the dark, and for what it is worth can be countered by pointing out that I have frequently succeeded with cases that have been intractable to psychoanalysis. It is likely that the statistical bias in such uncontrolled comparisons as might be made at present would favour the analysts, because while I accept every neurotic patient referred to me they frequently reject those not likely to benefit, exercising, in Dr Glover's words, 'suitable selection'.

Exact knowledge of the relative effectiveness of these two therapeutic approaches will of course be available only when their results can be compared in properly designed large-scale studies. But it may be said that the portents are none too favourable to psychoanalysis. The already available studies (e.g. Landis, 1937) that rather crudely compare the outcome of psychoanalysis merely with simple traditional methods fail to show any superiority for the former either in percentage or in quality of results. The fact that private psychoanalysts almost never publish their results and are usually very reticent about discussing them must also be regarded as significant, since in no branch of medicine do therapists make a secret of *favourable* results.

I should like to invite Dr Glover (or any other psychoanalyst) to try the procedures I have described in some of their more refractory cases of neurosis. If he could set aside his preconceptions sufficiently to do this conscientiously I am sure that even with little experience in the techniques he would have at least some pleasant surprises. Unfortunately, a remarkable attitude that psychoanalytic theory engenders towards the suffering of

^{*} This is particularly strikingly shown in the case of the systematic desensitization technique. Dr Glover alleges that the cause-effect relations are blurred by preliminary 'dressage'. But it is not my practice to tell the patient beforehand what is expected to happen with this technique. Furthermore, A. A. Lazarus has recently obtained excellent results with it in a group of patients to whom he gave no information at any time.

patients stands in the way. The logic of the theory leads to a minimizing of attention to symptoms. Thus when treatment has gone on for a long time (perhaps several years) without any alleviation of suffering the analyst can still cheerfully persuade the patient to have 'more of the same'—on the 'sure' grounds of his theory—a state of affairs without parallel in all medicine. (Often this is 'justified' by reference to some other 'gain', such as 'insight' or 'maturation'.) It is consistent with this that Dr Glover warns psychiatrists to be faithful to 'exploratory and interpretative methods' even when they are ineffective rather than try out methods such as mine. Don't let your patients' suffering as mine. Don't let your patients' suffering tempt you to stray from the faith!

Dr Glover shows a fine sensitivity to atmosphere in finding a similarity between my book and certain writings of half a century ago. As he says, attempts were then also made to understand behaviour, including neurotic behaviour, with reference to organic function. These attempts failed and were abandoned, not because they were wrong-headed and futile in principle, but because there was not at the

time sufficient detailed knowledge of neurophysiology and more particularly of the laws and conditions of learning. This left the field clear for the speculative theorizing of which Freud was the most ingenious exponent. The dilemma which until recently faced clinicians was succinctly expressed by Altschule (1957) thus: 'They may accept a theory of mental and emotional disorder that is based almost entirely on ancient speculations or they may wait for modern physiology and experimental psychology to provide the data for a valid theory of behaviour. If such data were available now there would be no need to choose—and no intuitive system, Freudian or otherwise, would receive any consideration.' My book purports to show that such data now exist, and that there is thus no longer any need for Freudian or other intuitive systems. This is a chilling situation for psychoanalysts to face and they deserve our compassion. Of course a great deal of research will be needed to expand the present small number of empirically confirmed postulates into a comprehensive and scientifically valid theory of neurosis.

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PSYCHOTHERAPY BY RECIPROCAL INHIBITION: A COMMENT ON DR WOLPE'S REPLY

By EDWARD GLOVER

Dr Wolpe's animadversions on my critique of his book, though under the circumstances perfectly natural and indeed quite consistent with the professional views he advocates, leave the issues existing between us precisely where they were. The matter and manner of his riposte do however afford me the opportunity of restating in a more condensed form the essential points in dispute.

My critical notice of his work was divided into three parts. The first comprised an assessment of the nature of his therapeutic methods, which, I stated, were quite patently rapport therapies to be judged and accounted for in the same way as any form of suggestion, hypnosis, persuasion or stimulus-therapy.* The second part consisted of an assessment of the theories by means of which Dr Wolpe sought to account for the effect of his technical procedures. Here I maintained that his obviously versatile employment of methods of molecular and molar suggestion vitiated any attempt to account for his results on a purely neuro-physiological basis. Finally an attempt was made to orientate the clinical psychologist regarding the practical and theoretical value of such neuro-physiological theories of mental function, using as a standard of comparison the working concepts of psychoanalysis. I had found Dr Wolpe's theoretical formulae inadequate to account for

either normal or abnormal mental function; and I find nothing in his reply to cause me to alter the position I have taken up. In short, neither my attempt to influence Dr Wolpe's fixed ideas by a single exhibition of reciprocal inhibition nor his attempt to alleviate by the same means what he takes to be my state of agitation at the severity of his threat to the 'citadel of psychoanalysis' seems to have had much therapeutic success, a conclusion which in any case will not surprise any reader familiar with the 'habit-formations' of psychological disputation.

Regarding the more subjective aspects of the controversy little need be said. In addition to diagnosing an agitated reaction on my part, Dr Wolpe holds that at certain points I have misrepresented both his technique and his theoretical views, that I misconceive 'learning theory' and obstinately disregard established therapeutic findings which, he believes, constitute a 'chilling situation for psychoanalysts'. In view of the provocation he endured (I had described his thinking as naïve and his understanding of Freud's conceptions as inadequate) this list of counter-aspersions is a comparatively moderate one. Indeed Dr Wolpe himself says, quite handsomely, that these are 'secondary matters', apparently deserving of 'compassion'. And here the factor known in legal circles as the 'credibility of the witness' might be allowed to rest with honours easy. But for one circumstance.

Dr Wolpe not only continues to support his theoretical views on the marked and apparently lasting clinical improvement he obtains in a high percentage of cases but, *à propos* the permanence of results obtained by non-analytical procedures, to impute to psychoanalysts the view that 'repression has a central role in the production and main-

* Incidentally, the use of manual contact at any point of any of these procedures in no way differs from the 'laying-on of hands' exclusively practised by some lay technicians and in the more remote past by those who 'touched' for the King's evil and other disabilities of a more psychopathological nature; for that matter the laying-on of electricity was a device commonly employed at the end of the last century in the treatment of hysterics and neurasthenics.

tenance of neurotic symptoms'. Incidentally even in the days before Freud reorganized his views on 'defence' and placed repression in a hierarchy of defence-mechanisms it was never held that repression either produced or maintained symptom-formation, but a 'fault' in repression caused by the introversion of libido and its subsequent regression to a point in the unconscious system at which the sum of excitations becomes pathogenic. To this he now adds, first, that according to my view 'differences between humans and animals should have *forbidden* the examination of any hypothesis *postulating* that they have similar basic mechanisms of *neurosis* formation': secondly, that I believe it to be 'the *prerogative* of psycho-analysts to achieve success with the more intractable and inaccessible cases'; and thirdly, that I 'warn psychiatrists to be faithful to exploratory and interpretative methods even when they are ineffective rather than try out methods such as mine' (my italics throughout).

Regarding the second and third of these imputations it is perhaps sufficient to say that they are borne out neither by the content of my critical notice nor by any of my previous writings on the subject either of analytical or of non-analytical therapy. As for the first, it may be remarked that in my view no consideration should be allowed to forbid the examination of any hypothesis. If Dr Wolpe is right, my critical notice was clearly an illicit attempt to do precisely what he maintains I myself forbid, namely, to examine his postulates and hypotheses. Indeed, this imputation is not only inaccurate but just a little ungrateful. For I was at pains to consider what improvement in rapport technique might account for a high percentage of satisfactory and permanent results, and plumped for his 'systematic desensitization' which depends on establishing thematic hierarchies of anxiety, pointing out at the same time that these epiphenomenological devices originally lay to the credit of the so-called 'hypno-analysts'. It is, by the way, intriguing to find that Dr Wolpe himself holds suggestion therapy in low

esteem, regarding its effects as 'often trivial and usually brief'. That is not at all my impression of the therapeutic claims of rapport therapists who, however, may well be left to defend themselves against the aspersions of a colleague whose own range of rapport techniques is unusually wide. In any case, and as Dr Wolpe seems to recognize, I have for many years advocated more scientific control of researches into the results of psychotherapy in general and psychoanalysis in particular. I believe that given accurate diagnostic methods and agreed prognostic and therapeutic criteria, including the assessment of so-called spontaneous cure, more dependable information on the subject could be secured, particularly regarding the phenomena of transference cure. But I fear that Dr Wolpe's existing standards of clinical definition would not qualify him to take part in framing the diagnostic tables. For unless some effective distinction is drawn between the basic mechanisms of a *functional* 'neurosis' and the structure of a '*psychoneurosis*', we shall never get anywhere.

But whatever the findings of future controlled research on therapeutic results may prove to be, they cannot alter the issue between Dr Wolpe and myself. The crux of the matter for the clinical psychologist lies in the theory of mind professed and the working concepts on which it is based. Dr Wolpe is apparently prepared on occasion to drop his neuro-physiological concepts for the purpose of clinical argument and to offer instead his version of 'learning theory'. The psychoanalyst keeps at all points of theory and practice to his basic working concepts and will continue to do so until he finds convincing reason to modify or abandon them. Dr Wolpe's work does not, in my view, provide any such reason. His conception of the 'physiological substrate' of mind is not only narrow but confusing. Using a structural image, he *jumps* the gap between physiology and psychology and in consequence says infinitely less about the relation of organic processes to mental function than do the

dynamic correlations advanced by Freud. As for his offer of 'learning theory' in place of dynamic psychology, this would, if unthinkingly accepted, set the clock back about fifty years. It would once again seal up and imprison psychological thinking within the protective cocoon from which Freud was the first to liberate it; the what and why and how and when of mind would disappear to make room for a rigid ideology of habit-formation;

the revolutionary distinction drawn and documented by Freud between 'psychic' and 'objective reality' would disappear and with it the wealth of information he amassed concerning the relations of man to the objects of his instincts; the sense of human sin would once more become a theological or at least ethical field of inquiry. This is too high a price to pay for the comforts of a mechanistic system.

ASPECTS OF CONSCIOUSNESS

BY A. PLAUT*

You may wonder why I have chosen to speak on a subject which is as complex and complicated and as ill defined as consciousness. I want to state right away that I cannot tell you what consciousness is, and to those who would approach a problematical term with requests for clear definition and precise localizations my subsequent reflections and speculations can be of little interest. The fact that dictionaries abound with definitions of consciousness is in itself an indication of the complexity of the subject, and it is therefore not surprising that the term has become somewhat disreputable among scientists.

In my search for common ground between the many contributory branches of 'medical psychology' (an equally ill-defined term) I came upon 'consciousness'. The purpose of my talk is to throw some light on the deeper nature of those difficulties which show up symptomatically as friction among psychologists whose orientation differs. Recognition of these difficulties may help to bring about the 'peaceful co-existence' which everybody wants and which is so elusive in practice.

I am therefore prompted by the same motives which were recognizable in Mr Alec Rodger's Presidential address to the British Psychological Society, 'Psychologists: non-medical and medical' (1958), as well as in Professor Ferguson Rodger's address, 'The other man's point of view' (1957). You will remember that the theme was also expressed by our retiring Chairman, Dr Russell Davis, in his exposition, 'Clinical problems and experimental researches' (1958). He mentioned how important it was that the right research questions should be formulated, and

my intention is to take up this cue and show *why* it is so difficult for the psychotherapist or analyst to formulate questions which could be tested in the laboratory.

The first major difficulty is of a personal nature and can be dealt with briefly: an analyst's work is of a consuming nature, i.e. it requires that he be whole-heartedly devoted to it. Therefore there is little energy left for what appears to be peripheral to his work. Moreover, he knows that he is dealing with mainsprings of human behaviour and suffering, and theoretical interest in psychological questions of less immediate urgency tends to flag. However, one also realizes—at least from time to time—that dealing with so much irrational stuff is apt to induce a certain slackening of mental discipline, inaccuracies in the only orienting instrument against which all one's work has to be checked, i.e. oneself. At such points analysts may find it necessary to undergo further analysis or to write a paper, or to look with some envy at the discipline and limitations of experimental psychologists, or any combination of these three.

The second major difficulty is inherent in the nature of our common ground, i.e. psychology, and I intend to demonstrate this, using 'consciousness' as my example. It should go without saying that I have to be selective according to the purpose of my discourse.

* * * *

About fifty years ago Angell (a pupil of William James in Chicago) wrote (1905, p. 1):

1. *Definition of Psychology.* Psychology is commonly defined as the science of consciousness.
2. *The Nature of Consciousness.* Consciousness can only be defined in terms of itself. Sensations, ideas, pains, pleasures, acts of memory, imagination, and will—these may serve to illustrate the experiences we mean

* Address from the Chair, Medical Section of the British Psychological Society, London, 28 January 1959.

to indicate by the term; and our best endeavour to construct a successful definition results in some such list, of which we can only say: "These taken together are what I mean by consciousness". A psychological treatise is really an attempt to furnish the essentials for such a catalogue.'

Two things are at once apparent when we look retrospectively at this quotation. First, until shortly before it was written consciousness was not seriously questioned as a unitary thing, co-terminous with mind. It was James who began to look at consciousness as a process, something in constant flux, his well known 'stream of consciousness'. Secondly, the moment the unitary and constant nature came into question its constituent parts were acknowledged as proper subjects of scientific investigation while consciousness (although surviving as a subject for review and symposia to our present day) lost its former prestige among academic psychologists. The second jolt came from the importance which 'the unconscious' gained through the discoveries of Freud. Consciousness in dynamic psychology became relegated to the position of a junior partner in the conscious-unconscious antithesis, and this antithetical concept has lost none of its practical significance since it was first applied. It is surprising that consciousness depleted of its unrivalled importance did not altogether wither under the two-pronged attack. I think this is partly due to the more conservative interest psychiatry continued to take in the description of various degrees of awareness and states of dissociation which could be studied when consciousness was disturbed. So far as I can see psychiatrists still find it useful to look upon consciousness as a whole, without asking too many questions about the nature of it. I mean as a totality forming the necessary background of ordered mental activity as distinct from separate mental functions (Klein & Mayer Gross, 1957, p. 75). The interest in consciousness was also revived by neurophysiology, especially as the result of electrophysiological investigations into sleep and

wakefulness in relation to mid-brain structures. More about this later.

Perusal of records of multidisciplinary symposia has made me aware that all attempts to learn another man's language—to use a metaphor—are fraught with the danger of translating it immediately into one's own. This seems to be an almost compulsive need. Although it may be stating the obvious, one cannot over-emphasize that the same words may have altogether different connotation when used in different fields of knowledge and at different times in history. Direct translation is therefore inadvisable.

An example may help: Edwin Boring, in *A History of Experimental Psychology* (1950), tells us that in Wundt's time, in the 1870's, there was a saying, 'No psychosis without neurosis'. To our ears this would mean that every psychosis presents some neurotic features. At the time it was coined the phrase meant no more than that every mental state had its accompanying neurological state, i.e. it was an expression of psychological parallelism! This is a digression, but it makes me wonder whether Sherrington was right in his belief that a transposition of physiological to psychological principles was not possible. While no one doubts the constant interaction between psyche and soma, many find it hard to maintain a double orientation in this field and not to interpret the one in terms of the other. A double orientation engenders a split in the beholder and we sympathize with his attempts to reunite the two images, body and mind, into a single one. From an ideal (monistic) point of view psyche and soma may be held to be one and the same thing: it is the advance in our knowledge of each which divides the two. To have knowledge means differentiation and implies a division between knower and object. (I shall have to come back to this point.)

When I thought of the nature of our difficulty in formulating questions for research by colleagues who work in the laboratory, I wanted to clarify for myself what the essential differences in our approaches and concepts

were. I only know about the clinical approach and hoped I could give a fair description and comparison of what the other kind of psychologist is attempting. Then I noticed that it was virtually impossible to do so without value judgements creeping into my description. The kind of thing I am referring to is well set out by Meehl (1954, p. 4), who describes himself as a hybrid working clinician and rat psychologist. I should like to quote: 'It is customary to apply honorific adjectives to the method preferred, and to refer pejoratively to the other method. For instance, the statistic method is often called operational, communicable, verifiable, public, objective, reliable, behavioural, testable, rigorous, scientific, precise, careful, trustworthy, experimental, quantitative, down-to-earth, hardheaded, empirical, mathematical, and sound. Those who dislike the method consider it mechanical, atomistic, additive, cut and dried, artificial, unreal, arbitrary, incomplete, dead, pedantic, fractionated, trivial, forced, static, superficial, rigid, sterile, academic, oversimplified, pseudo-scientific, and blind. The clinical method, on the other hand, is labelled by its proponents as dynamic, global, meaningful, holistic, subtle, sympathetic, configural, patterned, organized, rich, deep, genuine, sensitive, sophisticated, real, living, concrete, natural, true to life, and understanding. The critics of the clinical method are likely to view it as mystical, transcendent, metaphysical, super-mundane, vague, hazy, subjective, unscientific, unreliable, crude, private, unverifiable, qualitative, primitive, prescientific, sloppy, uncontrolled, careless, verbalistic, intuitive, and muddle-headed.'

This seems to me a fair and correct presentation so far as the adjectives are concerned, and as I want to avoid treading on either clinical or statistical toes on this occasion, I decided to give up my attempt and to look for an alternative, something more indirect than odious comparison and if possible more fundamental. Were there any common denominators, some structural elements (comparable to grammar in the study of languages)

discernible in the various views held by different disciplines concerning the vexed topic of consciousness? If the answer were in the affirmative, if despite all the vastly different methods and observations (as well as imagery employed in description) some common denominators could be demonstrated then—I thought and still hope—the origins and limits of all our knowledge may be reviewed and this in turn could throw some light on the nature of our difficulties.

* * * *

Let us look back to the time not so very long ago when introspection was the major technique of experimental psychology and the product of introspection was called consciousness. What has happened to introspection? As Boring (1953) points out, the modern equivalent of introspection persists in the reports of patients, in reports of sensory experience in psychophysics, in the phenomenological experiences of perception and in social psychology, etc. He adds that nowadays psychology simply employs the term consciousness whenever it finds the category useful for generalization of observations. This lowly status of introspection is also found in dynamic psychology which distrusts the method for true assessment of a subject's motives and has developed its own techniques. Nevertheless introspection (*Selbstbeobachtung*) forms a kind of permanent background to analytical work and enters of course into every report, replies to questionnaires, etc. What, one might ask, is therefore wrong with introspection as an exploratory method of consciousness? The answer would be easy to provide in terms of the unconscious, but would lead us around in a circle. I also suspect that the antithesis conscious-unconscious (even if the pre- and subconscious are included) will not prove to have such clear lines of demarcation as analytical practice makes us assume. We might therefore do well to examine some of the qualitative aspects of the hypothetical 'consciousness'. Here is an illustration:

Let us assume that one Monday morning

we meet a man who has just got out of his train on the way to work. We may stop him and ask a few questions and establish quite quickly that he can identify himself and his surroundings and he may even know what he had for breakfast, the correct date, the name of the Prime Minister, and the capitals of Europe. Someone may come to the conclusion that our man's consciousness is not disturbed and I shall not assume that he is deceived by some complicated fugue that may be going on. All the same, while he was travelling and since he got out of his train, the words of a song or bits of a poem heard perhaps long ago have been going through his mind, and for no apparent reason these words keep coming back. Again I am not going to assume that he is knowingly or otherwise in some state of distress, nor that the words of the song or poem are associated with repressed memories of a painful nature. If he should be in an introspective frame of mind he may discover that the words which have apparently come from nowhere are quite meaningful in relation to the task or a situation with which he will be confronted that day or that week. In fact the words may be regarded by him as a key to the situation with which he will have to deal and now their connexion and meaning have become quite self-evident to him, they have served an orientating function and, having done so, may sink back once more in oblivion. It was a subjective experience which he will probably keep private. So far as he is concerned it would be unnecessary as well as impossible to prove the meaning and importance of the incident to anyone else.

Let me complete my naïve illustration by the addition of a small variation: on another Monday morning a similar thing happened, but our friend's train was held up so he got a little agitated about being late for work, kept his eye on the clock and was much more aware of the time and the most expeditious way of getting to work, but paid no attention to the words which curiously had kept coming into his mind when he set out on his journey.

This commonplace event may serve to demonstrate some of the essential ingredients of consciousness and their relation to each other. It also shows why some subjective but interesting psychological experiences may never become suitable projects of investigation by means of laboratory methods. In the first place I was drawing attention to the concept of *focus* of consciousness, which is related to such qualities as alertness, awareness and attention. Our friend—on meeting the inquirer—was able to focus at request and so to satisfy some criteria of consciousness. He was also able to de-focus from the external immediacy of the situation and so note things out of the corner of his eye, so to speak, on which he could reflect. And by this reflection or introspection he was focusing on some inner event of which he was then able to make sense. That is to say, the phenomena of consciousness seem to be bound up with rhythmic alterations between focusing and de-focusing. This raises interesting questions: Are such phasic alterations in keeping with the natural organization of our central nervous systems? Does the increasing emphasis which we put on rationality constitute an interference with this rhythm by attempting to focus consciousness all the time?

Focusing or a high degree of paying attention makes for acute awareness in one direction while little or no heed is being paid to what is going on elsewhere. Awareness which by some has been regarded as almost synonymous with consciousness implies from another point of view a degree of unconsciousness. The focus narrows the field to a sharp point and neighbouring or distant portions are ignored. Anxiety seems to have the effect of narrowing the focus and shuts out other considerations, if I may generalize from my example. The circumstances of my story did not permit me to demonstrate a positive aspect attached to the narrow focus type of consciousness. It provides the necessary intensity of experience for the de-focused consciousness to reflect on and afterwards to make connexions with a wider area, thus

allowing such experiences to become integrated. De-focusing is therefore as vital an attribute of consciousness as focusing. If we bear this rhythmic fluctuation of consciousness in mind, the paradoxical difference between awareness and consciousness is not difficult to understand, and we may appreciate why a person who is fully awake is not necessarily conscious. If, on the other hand, I were to postulate that a sleeping man is not necessarily unconscious I should probably be suspected of making mystifying statements, statements which cannot be verified. Fortunately a physiologist, Kleitman (1954) from Chicago, has come to my aid.

* * * *

By putting a kind of silver-black make-up on the eyelids he was able to observe and photograph movements of the eyeball which occur at intervals during normal sleep; whenever such a person is woken up when the movements are noticed, he will report that he has just been dreaming, but not at other times. Dreams, argues Kleitman, depend on the cortex and dreaming is part of normal sleep pattern. The cortex—although relatively inactive during sleep—is also the essential organ of consciousness and dreams mean cortical activity. Therefore Kleitman can conclude that a sleeping person who is dreaming is not unconscious.

As an analytical psychologist I do not find Kleitman's conclusion about the consciousness of a dreaming person hard to accept, but I appreciate that many people would find his evidence more convincing than the kind of evidence I could advance. What consciousness means to Kleitman in his experimental work has probably very little to do with the way I use the term as an analyst. Yet it seems to me that we may both be intrigued by the fact that attention, discrimination and awareness, may describe activities of the mind which are distinct from consciousness. The different states of consciousness—whether in the physiological or analytical sense—which occur under the influence of anxiety, hunger, sleep, fever,

drugs, etc., are of great interest to both of us and may point to a common element in the knowledge which we acquired in our different ways.

Specific discrimination between objects or stimuli would seem to be only one attribute of consciousness, the focused kind, but several other aspects must now be considered.

It is impossible to speak about consciousness from the psychological angle without referring to the relationship between subject and object, ego and content of consciousness, the knower and that which is known to him. The curious fact that the subject can deal with an object which is its own identity and infer, with Descartes, its own existence from its own thinking, that the 'I' so to speak can turn around and look upon itself, has engaged many writers, even in recent times (Francis Schiller, 1952, Jung, 1954, Federn, 1953, to quote but a few). Nor is this division just a philosophical view: we know that when consciousness is disturbed, either by organic or psychological causes, there may be a loss of personal identity which may be total at first in that the individual does not realize that he no longer knows who he is. Then as recovery sets in he knows that he does not know. Or again in a hippocampal seizure, when the patient notices that all kinds of strange things are going on, that the totality of consciousness is disturbed, but can nevertheless assume the role of a detached observer (as has been summarized by Rapaport, 1951). Such observations seem to point to a comparable state of consciousness which we know from dreams in which the dreamer may dream that he is dreaming, or he may see the dream events appear as a play enacted on a stage. Conversely there are some dreams so vivid and convincing that even after waking the dreamer may wonder for a while whether the events did not happen in reality. I am quite aware of interpretations which may be made about degrees of vividness of dreams from the aspect of the unconscious. But for present purposes I should prefer to look upon the vividness of such dreams from the point of

the development or organization of consciousness, taking the view that the division of conscious and unconscious does not exist in infancy to anything like the same degree as in adult life.

One recalls that the original meaning of being conscious was to *know oneself together with the object*, i.e. in knowing any object one also knows oneself as knowing. However, this idealistic statement had to be revised (I assume in the light of our knowledge of the changing relations between subject and object in the course of infancy and childhood). All incipient consciousness seems to be of the narrow focus type and in the infant is characterized by a close linkage with bodily regions and experiences attributed to these. In this state there is little or no separation between subject and object. The knowing oneself together with the object is quite complete, yet from the point of view of a later, more adult state of reflecting consciousness, with its temporal and spatial differentiations, the infant is not conscious. Each step of progressive awareness is characterized by a separation from the previous state and it would seem to me that the rhythmic fluctuations between states of consciousness observed in the adult repeat at a faster pace these early fluctuations between being together with and separate from the object. However, a further developmental aspect has to be considered briefly. It is the naming of objects as a kind of communication that takes place between the individual and objects. When the infant begins to give the object a name, separateness between him and it has occurred. (This may be why the father is generally named by the infant before the mother, from whom separation is more difficult.) Finally as the result of these repeated separating experiences—by a process which may be compared with a positive feedback—he achieves a feeling of ‘I-ness’, a kind of being single and alone. In Federn’s terminology it is the ego-feeling which creates the ego. The principle which I am describing is that each progressive step in the development of consciousness is dependent on the

previous one being accomplished, but, and this is important, the previous state of organization does not altogether vanish. (In Piaget’s schemata, which I much respect, no allowances seem to be made for either fluctuations in the state of consciousness, nor for the continued existence of the previous level and method of communication between subject and object.) Taking dreams as an example we can say that there are three modes of communication: the way one just knows without being able to say how; secondly, imagery which may be visual, auditory, tactile, etc.; thirdly, language as an ordinary dialogue. The modes of communication as distinct from dream content suggest that developmental stages of consciousness do follow us around: just to know would correspond to being one with the object. Imagery already shows a certain amount of separateness and ordinary spoken language presents a further degree. The continued existence of a narrow focus type of consciousness in the healthy adult side by side with a de-focused, reflecting consciousness may also be seen in dreams. I look upon the detachment of the observer—indicated by such forms as ‘stage’ dreams—as akin to the reflecting type of consciousness (which I have elsewhere (1959) described as an attribute of the central ego nucleus), while the vividness and sense of reality of the dream, usually accompanied by a strong emotional reaction, corresponds to the narrow focus type of consciousness. The latter is comparable to an earlier stage of infant development, while the former reflecting type is the first to dissolve as we drift into sleep. At other times reflecting consciousness returns towards the end of the dream and may appear as the knowledge that one is dreaming. It may even be made use of by the dreamer who suffers from repetitive nightmares in order to lessen his anxiety by telling himself—while the nightmare is proceeding—that this is only a dream.

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What I have so far said will leave you in no doubt that consciousness is a complex and

complicated concept. To recapitulate: I drew attention to the two aspects which I called focused and de-focused, the former corresponding more to awareness and discrimination, the latter to reflection. I referred to the fluctuations between these qualities under varying environmental and developmental conditions and to the connexions between the unitary and divided states of consciousness, which are expressed in the relations between the knower and the known, between subject and object. The essential common element in all these attributes of consciousness is the division into two opposite aspects. Before continuing with my theme of the common elements I wish to end this section by pointing out one other very important attribute which I should like to call 'consciousness of choice' (in analogy with Kleitman's 'wakefulness of choice'). Earlier on I spoke about our interfering with the rhythmic variations of consciousness by putting too much value and emphasis on focusing. Yet each new focus implies that we are now looking at objects or events which from the preceding focal point of view may have been peripheral. The capacity to interrupt these variations and to direct the focus towards the periphery, towards the things which could only be noticed out of the corner of one's eye, is a special achievement of consciousness. Psycho-analysis by free association and analytical psychology by active imagination (Jung, 1954) have made use of this, and I think it is no exaggeration to say that it is as the result of this capacity of consciousness that 'the unconscious' has been discovered. It has helped us to set up standards and techniques for the assessment of subjective experiences which take in a much wider range of possibilities than the unaided method of introspection.

In addition to these common elements between different approaches there are other formal similarities, which emerge from a consideration of psychological as well as neurological models.

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About fifty years ago Baldwin (cf. Angell, 1905, p. 66) represented the field of consciousness graphically in the form of a target in which the 'bull's eye' was called 'the focal point of attention', the ring next to it was called 'active consciousness', followed in turn by 'diffused, vague consciousness', 'the sub-conscious', and 'the unconscious', represented in the outer circle. This presents a hierarchical order of mental functions comparable with the neurological levels of Hughlings Jackson. By referring to the centre as the bull's eye I am also drawing attention to the implicit value judgement of the model corresponding on the neurological side to the 'highest' functions.

The point about such hypothetical models is that they are bi-polar; two opposite qualities at either end and the intermediate degrees marked on the connecting line. By contrast with these models which are essentially based on a division of a concept into two opposite entities, I should like to give my rather condensed version of Jung's model (cf. 1954) which is based on the number three.

Consciousness seems to Jung a hypothetical construct which serves to understand under what circumstances some contents are accessible to us and others not. Contents may become subliminal, i.e. fall below the threshold of consciousness for two reasons. One is because they are repressed, as is borne out by observations of psychopathology, e.g. symptoms are the indirect representations of such unconscious contents which are, however, potentially capable of becoming conscious. All conscious and potentially conscious contents are regarded as belonging to the psychic sphere.

Just as in the previous model, the psychic sphere is conceived as a connecting line with limits at either end. Beyond these limits are physiological driving forces at one end and regulating patterns, better known as archetypes, at the other. Neither can be directly observed; they can only be inferred. However, the terms do not matter in the present context. What is important is that a third

entity, an observing 'I' or ego, capable of focusing on the middle portion, the psychic sphere, is introduced. The conscious ego may be thus presented as a virtual point at the apex of a triangle. The localization of this point at any given moment depends firstly on its relations to the contents of the psychic sphere and secondly on influences impinging on it from the two entities, which are as I said conceived to lie beyond the psychic sphere at opposite ends. This trinitarian schema of consciousness bears an obvious resemblance to the Freudian Id, Ego and Superego.

The formal principle or common denominator in its most abstract form is the number three or, in special terms, a triangle. The exact qualities, degrees, or functions of the concepts *within their own systems* are not relevant here. As long as I am sure that both models purport to explain approximately the same phenomena I feel free to compare the structural elements on which both seem to be based.

It is of interest that in recent times a neurological model based on three regions has emerged: in addition to the neo-cortex, which has for long been associated with the phenomena of consciousness, the old cortex or Limbic System (better known as rhinencephalon), has been shown to be associated with primitive affects and patterns of behaviour so much so that this part has been called the 'visceral brain' by Maclean (1955). Another, third region or better connecting system called by Penfield (cf. Penfield & Jasper, 1954) the centrencephalic system, functions as an integrating or co-ordinating system between the other two, the new and the old cortex. These three connected systems have been shown to be associated with manifestations of dissociated consciousness, e.g. in certain kinds of epilepsy. At any rate, it is well established that the most differentiated region of the brain, the new cortex, cannot maintain the functions of consciousness unless activated by the co-ordinating system just mentioned. The only point I wish to make here is that another model of consciousness based on the

number three has emerged in a different discipline.

I hope to have made it clear that by comparing structures and formal relations I am not talking here about analogical brain models, which all too easily persuade one to unite what our advancing knowledge has divided. It is one thing to borrow imagery for the purpose of description. The use of analogy in order to fill in a gap in one's own knowledge is quite a different matter. If I have failed to do so let me quote what a pioneer in neurophysiology, Lashley, had to say on the matter when Penfield's paper on memory mechanisms was discussed (cf. Penfield, 1952, p. 195):

'I am less impressed with the analogies... such as are discussed in cybernetics. There has been a curious parallel in the histories of neurological theories and of paranoid delusional systems. In Mesmer's day the paranoic was persecuted by malicious animal magnetism; his successors, by galvanic shocks, by the telegraph, by radio, and by radar, keeping their delusional systems up-to-date with the latest fashions in physics. Descartes was impressed by the hydraulic figures in the royal gardens and developed a hydraulic theory of the action of the brain. We have since had telephone theories, electrical-field theories, and now, theories based on the computing machines and automatic rudders. I suggest that we are more likely to find out how the brain works by studying the brain itself *and* [italics mine] the phenomena of behaviour than by indulging in far-fetched physical analogies. The similarities in such comparisons are the product of an over-simplification of the problems of behaviour.'

In other words I would agree that analogies have their uses but the greatest caution is required not to mistake analogies for explanations. It is a different matter when we look at common structural elements of models of consciousness arrived at by different methods of study.

I therefore ventured to put some formal (topographical) speculations before you. These speculations lead us to the roots of our know-

ledge which are not rational but have, for better or worse, a profound influence on our planned thinking processes. Numerical abstractions must be considered here: the number one is hardly recognizable as a number as it still presents unity. Two marks the first division, but it is still only 'the other'. While there are only two, one might still hope to go back and re-unite into one. This would correspond to the attempts of translating the other man's language into one's own as I pointed out above. The three, however, makes one and two clearly recognizable, it divides and paradoxically unites by creating a different order. To those who find it easier to think in terms of family groups the third would correspond to the arrival of the first child, who divides or differentiates the parents and creates a new group. It is no coincidence that Freud's major contribution to consciousness was based on his recognition of the importance of the Oedipus myth, an eternal triangle. And Jung made interesting observations concerning the number three in relation to his interpretation of the Trinity and to consciousness.

I shall try to illustrate by continuing with the example of the family group: awareness of incestuous desires between child and parent and the concomitant rivalry and hatred between child and parent of the same sex, and all the complications and anxieties to which this situation may give rise, certainly constitutes an increase in consciousness. Jung's extension of the Oedipus schema may be summarized as follows: man and woman contain within their psyche (and body) characteristics of the opposite sex, but these characteristics are for various reasons and in different ways disowned by them. If, for example, a son is born to a couple, the mother not being affected by oedipal rivalry may find the new situation by oedipal rivalry may find the new situation more agreeable than her husband. By way of (unconscious) identification with the boy's maleness the sense of her own worth may increase, and her hitherto disowned male characteristics may begin to function more positively, i.e. in the form of self-assertion (as distinct from negative aggressiveness). If her

husband is conscious enough of his son as a rival and therefore not the victim of too much unconscious jealousy, he may welcome his wife's increased self-assertion. A complementary situation—by which the father's female receptive aspects could develop—would arise if a daughter were born to the couple. I am painfully aware of the inadequacy of this description and cannot over-emphasize that what I have given is only one of many possible schematized illustrations of a quaternity: mother—son—father—daughter, symbolizing completeness. It would be obvious nonsense to take it literally in the sense that only couples with children of both sexes have the opportunity of extending and stabilizing their consciousness in the way described. Nor should the integration of characteristics associated with the opposite sex be regarded as more than one example of symbolic wholeness.

That consciousness can operate as a whole is borne out by experiences (e.g. the *satori* of the Zen Buddhist) when all the modalities of consciousness come together in one unifying and enlightening flash. Consciousness may here be synonymous with integration. Analysts may compare this with those rare incisive interpretations which are suddenly created out of a specific moment. A further reflection on the ultimate origins of our knowledge of consciousness is the light and sight symbolism which crops up regularly when consciousness is reviewed, or one tries to throw light on previously unknown things, or one has an illuminating thought, etc. It is in the nature of such imagery that its validity cannot be tested in the laboratory.

However, an interchange of knowledge between experimental and laboratory methods has proved profitable in the past and may do so again. Fechner (cf. Boring, 1950, p. 290), for example, arrived at what he called 'negative sensations', i.e. sensations which were subliminal, as a result of experiments, and by means of a mathematical formula. He also wrote (cf. Jones, 1956, p. 410): 'What is below the threshold *carries* the consciousness, since it sustains the physical connection in between.'

He likened the mind to an iceberg which is nine-tenths under water and whose course is determined not only by the wind which plays over the surface but also by the currents of the deep. Ernest Jones, in his biography of Freud, tells us how highly Freud thought of Fechner. Jung's association experiments, which he carried out before his interest became devoted to the study of symbols, are also well known.

Speculations arise from the matrix of consciousness and from these will spring new

hypotheses which may be suitable for testing. At the moment I can only hope that the formal resemblances between conceptual systems may offer an alternative to efforts at unifying different but complementary approaches to psychology until such time as a common theoretical basis has come into existence.

Speaking as an analyst I would say that if we are prepared to submit our theories of the unconscious to constant re-vision, 'the stream of consciousness' will continue to flow.

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THE RELEVANCE OF BODY IMAGE TO NEUROSIS

By DENNIS G. BROWN*

'But it's no use now', thought poor Alice, 'to pretend to be two people! Why, there's hardly enough of me left to make *one* respectable person!'

Alice's Adventures in Wonderland (1865).

The organic origin of the concept

A short while after Lewis Carroll had described the predicament of the shrunken Alice, Bonnier and Pick were describing disturbances of awareness of the body in cases of organic brain disease. A little later, in the first quarter of this century, the concept of body image (or schema) was given an important place in the writings of the neurologist Henry Head (1926). He postulated that each individual slowly acquires, by the cortical organization of afferent impulses, plastic models of himself against which to judge changes in posture and other experiences; and that though usually unconscious, these schemata 'may be manifest in the form assumed by sensations or images'. Great interest in neurological syndromes involving disturbances of body image continued on the Continent, and Paul Schilder was among those actively involved in the problem of localization of this cerebral function. In addition he was a protagonist of the usefulness of the concept in the study of psychotic, neurotic, and normal behaviour and experience. In *The Image and Appearance of the Human Body* he wrote (1935, p. 300): 'The postural model of the body is not merely a psychic addition to the solid structure of the body, but also a physiological entity with physiological consequences influencing the functions of the organs and possibly also their form and growth.' Schilder recognized the complexity of the concept, and stated that it is related to ideas as well as perception. More recently, Scott (1948) has defined Body Schema as 'that conscious or unconscious integrate of sensations, percep-

tions, conceptions, affects, memories and images of the body from its surface to its depths and from its surface to the limits of space and time'. Smythies (1953), to avoid the looseness entailed in such a complex concept, has urged the use of several separate terms: 'body image' being retained for only 'a visual, mental, or memory image of a human body', one's own or someone else's; while 'body concept' would signify each individual's constellation of memories and beliefs concerning his own body (independent of direct observation, and affected by wishes, pride, etc.); 'body schema' would imply the unconscious system involved in posture and muscular co-ordination; and 'the perceived body' would stand for our awareness of our own body as a spatially extended field. The perceived body, situated in the brain, is organized through the relating of sensory impulses from the physical body to unconscious body schemata, which modify them in terms of what has gone before. While Smythies' distinctions are of importance for experimental and philosophical exactness, possible overlap and inter-relationship call for a cautious and open approach. In this paper the traditional term 'body image' will be used largely in his sense of 'perceived body'.

Recently Bennett (1956) described an experiment comparing the errors in perception of the vertical (depending on the integration of visual, tactile, and postural perceptions) in normal subjects, schizophrenics, cases of organic brain damage, and a heterogeneous group of patients with disturbances of body perception (depressives, schizophrenics, paraphrenics, and hysterical psychopaths); and showed a tendency for the body-percept cases

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to perceive alike, and with similar errors to the organic cases. This is surprising, but important if confirmed, and of especial interest to us in view of Mayer-Gross's views on depersonalization. This symptom, which many, including Schilder, relate to a generalized body-image disturbance, has been considered by Mayer-Gross (1935) to be a preformed cerebral dysfunction, with a mechanism comparable to those of epilepsy, delirium, and states of semiconsciousness. He points out that depersonalization has been reported in such diverse organic conditions as epilepsy, brain tumour, encephalitis, arteriosclerosis, and botulism; but that such cases are uncommon compared with depersonalization in anxiety states, hysteria, obsessional neurosis, and neurasthenia (as well as endogenous depression and schizophrenia), which it may represent as a non-specific *forme fruste*. Depersonalization, which Schilder believed to occur at some stage in the development of nearly every neurosis, has been held by Galdston (1947) to represent the patient's perception of a less integrated state of ego-functioning. The main thesis of this paper is that such a view is not incompatible with a neurophysiological approach.

It is generally believed, in so far as localization is possible, that the body image is topographically related to the posterior part of the parietal lobe, especially of the right (non-dominant) cerebral hemisphere, and its underlying area of thalamus; though as Macdonald Critchley states in *The Parietal Lobes* (1953): 'Such is more probably an activity of the brain as a whole, one which is inherent in the personality, and which cannot logically be localized to this or that region of the brain.' Gross lesions in this area produce, most commonly, unawareness of hemiparesis (anosognosia) or loss of awareness of one body-half (hemidepersonalization), but also subjective disappearance of the body (total depersonalization), undue heaviness or swelling of part of the body, and elongation or separation of limbs. Though one would expect the changes in neurosis to be much subtler, it is of interest

to note the interaction of such gross disturbances with the patient's personality. In connexion with anosognosia, Critchley says that of patients who admit paralysis, some show little or no distress, some may attribute it to a local lesion in the periphery, and some may realize the trouble and develop severe reactive depression. Those who obstinately deny paralysis show other psychiatric features, from confusion to 'explanatory' paranoid reactions, some even thinking the limb belongs to someone else. Organic repression of disease has been related to personality factors, among others, by Weinstein & Kahn (1950); some of their patients, with a background of insecurity and a strong drive to superiority, reacted to the illness with anxiety and guilt; some, with a meticulous obsessional personality, became euphoric; some, basically more aggressive, became paranoid. Whilst these gross (and usually transient) disturbances of the body image are mostly caused by cerebrovascular lesions of the non-dominant parietal lobe (or less commonly by tumours), mutilation of the body, such as amputation of a limb, also involves a reorganization of the schema. Haber (1956) has pointed out that individual variation in the phantom-limb phenomenon, in regard to vividness, dimensions, tendency to fade and shrink with time, and the unequal fading of distal parts, and illusions of moving, all suggest a central process rather than a purely peripheral one.

In addition to gross structural changes, 'functional' disorders such as epilepsy (Stevens, 1957), and migraine (Lippman, 1952) are known to produce body-image changes. Indeed poor Alice's shrinking and enlarging experiences in Wonderland may have been descriptions of Lewis Carroll's own mundane migraine. Vivid and often disruptive distortions of the body image may be seen in schizophrenia. They, like the delusions of deadness and decay in melancholia, are often accompanied by failure to identify with the environment, which appears unreal. Derealization and depersonalization are interrelated and

often interchangeable, a fact of importance in view of the physical body's position between the outer and inner worlds, and between the environment and the perceived body (body image).

As Bennett suggested, the neglect by clinicians of the body image may be due to unfamiliarity with the problem, the reluctance of patients to speak of such experiences, or to the difficulty of distinguishing between delusions and true disturbances of perception. However, distortions of the body image are universal.

The flexibility of the body image in normal people is seen in the changes that occur when riding in an accelerating or decelerating lift; in the sense of disappearance or distortion of the body in fatigue, falling asleep and wakening; in the dissolution of body boundary in sexual orgasm, or some aesthetic or mystical experiences; and in experience under drugs such as mescaline and lysergic acid diethylamide. Later, work will be mentioned that raises the body image to a central position in normal and neurotic personality development and functioning. Thus, though the origin of the concept lies in descriptions of organic brain disease, it is applicable to a wide range of conditions, organic and functional, pathological and normal, and forms a bridge between them.

A psychophysiological focus

In *The Organization of Behaviour*, Hebb (1949) describes the central problem of both psychology and neurophysiology as: 'Psychologically, it is the problem of thought: some sort of process that is not fully controlled by environmental stimulation and yet co-operates closely with that stimulation. From another point of view, physiologically the problem is that of the transmission of excitation from sensory to motor cortex.' Two concepts, the Ego and the body image, occupy some such central position, and it is felt that a study of their inter-relationship will facilitate a psychophysiological approach to the problems of neurosis.

In his development of the idea of psychic structure, involving Ego (self), Superego (conscience), and Id (instinct), Sigmund Freud emphasized that the ego is first and foremost a body-ego, a mental projection of the body surface derived from bodily sensations. He wrote in *The Ego and the Id* (1923): 'This ego includes consciousness and it controls the approaches to motility, i.e. to the discharge of excitation into the external world; it is this institution in the mind which regulates all its own constituent processes, and which goes to sleep at night, though even then it continues to exercise a censorship upon dreams.' The ego is, in the words of Hartmann, Kris and Loewenstein (1946), a specific organ of adjustment. Paul Federn (1953), who gave the concept of the ego a key position in his psychology, spoke of ego-feeling as the feeling of bodily and mental relations as a unity in space, time, and causality. Though he distinguished between mental and body egos, because in dreams they are felt separately, he held that in the normal waking state the mental ego is always experienced as being inside the body ego. He also distinguished between the body ego and Schilder's body image on the grounds that the entire body may not be invested with ego-feeling; but he was aware of the abnormality of such deficiency, prompted as it is by instinctual conflict. Winnicott (1954) expresses something similar when he states that the mind does not exist as an entity, provided the psyche-soma or body scheme develops in a satisfactory environment; and that overgrowth of mental functioning occurs in an attempt to deal with an environment unsatisfactory to the psyche-soma, and becomes the basis of a 'false self' usually situated inside or just outside the head.

Neurosis is a state of faulty adaptation of the organism to its environment often precipitated by psychological stress, and involving the experience of characteristic syndromes of symptoms (mental and/or physical) amongst which overt or covert anxiety is prominent. The causes of the maladaptation, or susceptibility to psychological stress, are

often unknown to the sufferer, and the early onset and history of neurotic traits in childhood point to a constitutional or developmental origin. The term *neurosis* originated in the nineteenth century, when these conditions were thought to be due to some disturbance of function of the central nervous system or peripheral nerves. When conditions were found in which such dysfunction was not apparent, they were described as *psycho-neuroses* to distinguish them from 'true' *neuroses*. The thesis supported here harks back to the original concept of the *neuroses*, without ignoring the advances in psychology, and attempts to transcend the Cartesian dualism of mind and matter. It is that in the relationship of ego and body image, as organized in the cerebral cortex (perhaps the parietal lobe), we may have an important link in at least one causal chain underlying the clinical picture we call *neurosis*.

Without explaining how, Schilder held that in addition to perceptions and the actions based on them, emotional influences play an enormous role in the final shaping of the body image, changing the relative value and clearness of different parts; and that just as organic processes have a centripetal influence on the body image, 'the mental problems and libidinous conflicts of the *neurosis* lie in the centre of the personality and flow from there to the periphery of the personality and into the postural model of the body', i.e. have a centrifugal influence. However, Callaway & Thompson (1953) have demonstrated that increased endogenous sympathetic activity, like introversion (Singer, 1952) and schizophrenia (Weckowicz, 1957), decreases size constancy. This supports their hypothesis that increased sympathetic activity decreases exteroceptive input. Interoception may be similarly influenced by the changes in autonomic balance that occur in *neuroses*. Modern research has illuminated the inter-relationship of cortex and reticular system and the problems of attention and sensory filtering (Hernandez-Peon *et al.* 1956). In experiments on patients with multiple electrode implants, Gray Walter (1959)

has detected striking individual differences in sensory filtering, and has suggested that impoverishment of adaptive repertoire can result from anomalies in the functional relations between the limbic cortex, diffuse projection systems, and hypothalamic-autonomic mechanisms. It may be that cortical organization of the body image is involved in these inter-relationships. As Lashley (1954) has said: 'The selective character of attention is a result, not a cause of the organization of percepts.'

The ego in psychopathology

In the analytic view, the crux of neurotic conflict is the failure of the personality to adjust to the deprivation or postponement of instinctual satisfaction, in accordance with outer reality. This failure of ego function may result from domination by a harsh super-ego, as in depressive or obsessional *neuroses*, or by a revolutionary id in hysterical or psychopathic states. Much disagreement between psychoanalytic schools stems from differing evaluations of unconscious factors, rather than divergences from this ego-centred tripartite structure. Thus 'orthodox' analysts follow Freud in regarding the genesis of *neurosis* to be the active repression of early anxiety-provoking experiences, so that they merge into the unconscious id; and the treatment to be the release into consciousness of this material and the associated emotions, and its integration into the ego. Alfred Adler laid little stress on extensive analysis of the unconscious; and regarded *neurosis* as a conflict between individualistic and social drives, expressing itself as a faulty style of life, and originating in an attempt to compensate for the child's real weakness and inferiority. 'The body concept' appears to be of more relevance to his psychology than 'the perceived body' (Smythies), and the Adlerian system could in that sense be regarded as less profound than some, though a useful basis of treatment in many cases of *neurosis*. The same may be said of the American sociological school (e.g. Sullivan and Horney) which it led to by its emphasis on the careful study of the

total life situation. Sullivan in particular, by emphasizing interpersonal relations rather than instinct, by recognizing that one can have interpersonal relationships with illusory persons, and that such abnormal relationships can persist from infancy, adumbrated the British School. Both Melanie Klein and W. R. D. Fairbairn developed the idea of object-relations. Perceived part-organs of the parents, and later the whole parent, become introjected and incorporated into the developing psyche, and split into good and bad aspects (objects). Klein has not changed Freud's tripartite structure, though her work indicates that such bad objects can be cruel precursors of the later moral superego. Fairbairn has altered it, eliminating the id on the grounds that it represents instinctual *forces*, not structure, and incorporating aspects of it ('libidinal ego') in a more complex unconscious ego. He regards psychopathology as a study of the relationships of the ego to its internalized objects. This does not concern us directly here, but the whole concept of introjected objects being incorporated into the structure of the ego at first seems to threaten the identification of the primal (body-) ego with the body image. However, these perceptions of the parents or their parts are registered and evaluated as satisfying or unsatisfying ('good' or 'bad') by a developing organism, in whom different organ systems dominate as differentiation proceeds. Thus object-relations and the perceived body are integrated from the beginning.

The relationship of neurosis to ontogeny

It will be recalled that medullation of cerebral neurones in the last few months of intra-uterine life proceeds in the following order: those subserving touch, proprioception, smell and taste, vision and hearing; and that at birth, cerebral motor tracts are still immature, and association fibres are the last to be medullated (Conel, 1939). Freud's postulation of three phases of personality development, oral (early and late), anal, and genital (pre- and post-latency), is widely accepted, though the

American sociological school and Fairbairn have rejected the anal phase. It seems likely that adequate gratification of tensions in succeeding phases of development may become generalized to a feeling of total body adequacy. Experience in the first year of life is almost totally confined to the body. D. W. Winnicott has emphasized the importance of early tactile and proprioceptive experiences, such as contact with mother and being held and nursed; and Erik Erikson has related differences in such nursing techniques to characteristic personality patterns of different cultures. However, incorporative experiences are of great importance. During a period when the major part of waking life is taken up with feeding, satisfaction and frustration may well be the earliest challenge to the omnipotence of instinct and phantasy, and the origin of the self-nonsel self distinction; that is, the beginning of the ego as an organ of adaptation between instinct and reality. Isaacs (1952) has suggested that phantasy (unconscious mental activity that may, however, become conscious), accompanies all instinctual processes. Thus a link can be seen between the perception of these early incorporative activities, and the introjection of aspects of the mother into the developing body-ego, in accordance with the child's satisfying or unsatisfying experiences of her. The development of the sense of self along with aggressive potential, demarcates the early and late oral phases in the first year. In the second and third years of life the experience of the child is immensely enlarged by motor and visual exploration of the environment and the development of verbal communication on the basis of auditory perception. These bring the developing child into an enlarging and more complex environment, necessitating greater control over bodily functions and instinctual demands, and the relating of the self to more people. This is the period of Freud's anal phase (involving the control of automatic activities in accordance with parental approval and disapproval) and Fairbairn's transitional phase (involving growing from one-sided dependent relations

to two-sided ones). This leads to the genital phase, with its problems of sexual identification and rivalry, which is soon forced into latency until adolescence. Though differences still exist, most analysts agree that both schizoid and depressive states are related to adjustments in the first year, respectively to the early and late oral phases; while obsessional states, hysteria and phobias, as well as paranoia, result from conflict in subsequent phases. If an individual is fixated or regresses to an earlier phase, he has to deal with adult experiences in terms of a body image appropriate to simpler, childhood experiences.

The role of body image in cortical functions

Professor G. P. Meredith of Leeds University (1951) has emphasized the part played in the formation of language by spatial and temporal abstraction from the flux of perceived experiences. The former is the basis of nouns, the latter of verbs, while both factors enter into the development of adverbs and prepositions. (The qualities expressed by adjectives, including value judgements, are based on parental evaluation.) Thus language, the basis of intellect, is grounded on the organization of a framework for judging spatial experiences in regard to discreteness and sequence. Meredith notes that the child's body is intermediate between the environment and the inner world. It provides sensory data about the outer world and the child's own body, but also the interoceptors give a general affective tone to the inner world, while the proprioceptors report on the spatial orientation of the body and structure of the inner world. This structure, he holds, as well as being a foundation of rationality, is intimately related to the body ego. Clinical support is given to this view by the direct observations made by the late Elizabeth Norman (1954) of cases of childhood schizophrenia and early infantile autism. She concluded that a failure in body-image organization underlies the children's preoccupation with the perceptual properties of objects and words, rather than their function and use; and is the

basis of their difficulty in distinguishing between the internal and environmental stimuli and attaining a sense of self-identity.

It might be objected that neurological evidence indicates that the right parietal lobe tends to show spatial specificity, the left verbal specificity; i.e. body image and the verbal basis of intellectual activity appear to be related to opposite lobes. However, considerable functional overlap appears to occur. Gerstmann's syndrome, which usually occurs with lesions of the left parietal lobe, is characterized by finger agnosia and right-left disorientation, as well as difficulty with writing and arithmetic, suggesting that various basic skills and capacities may be dependent on a well-organized body image. A recent paper by Klein & Ingram (1958) described a woman with a tumour of the corpus callosum and right parietal lobe, who showed unawareness of the left side without motor or (probably) sensory loss, suggesting that the lesion prevented utilization of intelligence by the affected limb, as well as causing a disturbance of the body image. Critchley has pointed out that some workers (e.g. Nielson and Dattner) believe the two parietal lobes are equipotential in regard to the body image, but that lesions of the left one produce more obtrusive disorders of speech and praxia, which obscure the body-image disturbance.

Symbolization, a cerebral function greatly enriched by language, is important in neurotic processes as the mechanism of substitution and displacement: the former being the use of another bodily organ instead of the one which affords feared or condemned gratification; while the latter refers to an emotional response or general activity, in a situation other than that really causing the response. In a stimulating article, Kubie (1953) has discussed the central symbolization of the body organs in health and disease. The body image has been related primarily to proprioceptive and exteroceptive afferent impulses, and to ego functions. But Meredith pointed out the affective tone contributed by interoceptors and Kubie has described the contri-

bution of other organs to the central representation of the body. He classifies this symbolization as:

I. *Organs implementing our relationships to the external environment*: the exteroceptive sense organs, striated muscles and their proprioceptive controls and the organs of speech—chiefly innervated by the central nervous system.

II. *Organs of internal economy*: lying deep inside the body—and chiefly innervated by the autonomic nervous system.

III. *Organs of primary instinctual function*: orificial organs, which exchange substances with the outside world continuously or periodically as a result of deprivation or accumulation—in which functioning is started by central nervous system but finished by autonomic nervous system action.

IV. *Involving body image as a whole*.

(I) he regards as 'organs of the ego', occupying a clearly defined place in conscious and preconscious thought; they belong to the original body self, but because they mediate our relations with the environment, they can represent the nonself world on an unconscious plane. Disorders of these organs are classified as conversion hysterias. (II) do not usually occupy a place in consciousness unless we are ill, despite the fact that they are an essential part of the self; unconsciously they may just as likely be viewed as belonging to an alien world. Disturbances of these organs are usually spoken of as 'organ neuroses'. (III) are represented in conscious and unconscious symbolic processes concerning the orifices, on the basis of appetites and drives shaped by neuromuscular and glandular activity; they may be regarded as organs of the id. The instincts are bound up with homeostasis, and the appropriate orifice must be represented psychologically to ensure adequate function, gratification, and tolerance of frustration. Disturbances of symbolization of these organs would seem to have a close connexion with neurotic development. (IV), involvement of the whole body image in the

psychosomatic sphere, is seen in chronic invalids, but also in people passing to and from other groups. Kubie asserts that every conceptual unit is rooted in both self and non-self worlds, and adds: 'In this way the symbol itself may constitute the bridge between these alternative and often simultaneous channels for the expression of internal tensions. In other words, it is the symbolic process, with its multipolar conscious, preconscious and unconscious linkages, which provides us with projective pathways for language and distance imagery at the one end, and introjective pathways for somatic dysfunction at the other end.' As a provocative oversimplification one could regard neurosis as resulting from an imbalance in the representation of organs in groups I and III.

Recently, in a thoughtful speculative paper, Rosenzweig (1958) has identified the reticular system (together with diencephalic and rhinencephalic structures) with an Affect System which predicts the future in terms of past percept patterns and the current or anticipated internal state. This process is consciously represented as emotion, and accompanied by phantasy of the desired or feared object. Activating percepts may be interoceptive or exteroceptive stimuli, and the affect system cannot distinguish between them as it is not concerned with ego boundaries or body image. Rosenzweig believes that neocortical development (the Abstract System) corrects this deficiency, providing for more exact definition and correlation of percepts and their relation to a body image which distinguishes between internally and externally derived stimuli. He postulates that Freud's primary thought processes (associated with unconscious and instinctual mentation) are associated with the reticular system, and his secondary thought processes (associated with conscious intellectual mentation) are associated with the neocortex. Modern physiological and anatomical knowledge permits some such thesis. Le Gros Clark (1958) in a recent lecture emphasized the important influence on perception of central regulative mechanisms acting through centri-

fugal nerve fibres at every level of the sensory input. Further, in striking accord with Rosenzweig, he speculated that the reticular system is concerned with intuitive and instinctual processes. Here then we have a way of looking at the concepts of ego and id in neurophysiological terms, which supports the notion that the body image plays a vital part in the relating of instinctual experiences to external reality. Processes of repression, conflict between ego and id, and ill-adaptive identification (as seen in an oversevere superego), may all be expressed in terms of poor adjustment of the crude instinct-dominated affect system to neocortical reality-testing and 'self-identity'. It might be expected that such conflict could be reflected in the body image.

Evidence of the influence of body image

There is evidence that the body image affects both perception and behaviour. In *The Interpretation of Dreams* (1900) Freud quoted Scherner's early observation on the effect of organ-stimuli on dreams. On the basis of experimental techniques, Werner & Wapner (1949) have put forward a sensory-tonic theory of perception, which depends on an equilibrium between the effects of stimulation from an object and the existing distribution of body tonus; perceptions being such as to disturb the existing tonus patterns minimally. Wittreich & Grace (quoted by Fisher & Cleveland, 1958) have shown that normal subjects, when describing their own mirror images distorted by wearing aniseikonic lenses, emphasize minor details; whereas in describing other individuals similarly distorted, they emphasize over-all size changes or body tilt. Subsequently, in a study comparing psychiatric patients with controls, it was found that the psychiatric patients described distortion of self-image in terms of over-all size, i.e. as normals describe distortions of others. They suggested that normal individuals have a more detailed body image, which 'functions differently in different environments and roles'; and that inflexible and inappropriate behaviour of psychiatric patients may be partly due

to the undifferentiated and rigid body image. Karen Machover (1951) has developed drawing of the human figure as a projection test, evoking feelings and conflicts concerning the body, but in which all of Smythies' facets of body image interplay. In an important study (Witkin *et al.* 1954) figure drawing has been used to evaluate ability to maintain spatial orientation in a number of situations where cues regarding spatial position had been distorted or removed. It was discovered that this ability was greater in subjects who relied on proprioceptive cues than in those who relied more on cues from the immediate field situation. It was found further, that the figures of the field-dependent group reflected a low evaluation of their bodies, and emphasized like sex traits; while the proprioceptive group appeared to value their bodies highly, and included enhancing traits from both sexes. It was concluded that the ability to deal with unstructured spatial situations was related to the individual's attitude to his own body. Zazzo (1956) has shown that spontaneous recognition of one's mirror image is learned, long after the body image (perceived body) has become a functioning entity and that it is temporally related to the development of language. Recognition first occurs at about two-and-a-quarter years, and is preceded by a kind of disorganization, and followed for several months by a combination of anxiety and pleasure, before the child settles down to an easy acceptance of his mirror image. It may be that during this time an integration is occurring between different aspects of the body image (body-concept, -schema, -image, and the perceived body). The psychosocial implications of physical appearance are not our direct concern here, but the relevance of looks to self-identity and social identification are obvious. It would be interesting, if possible, to relate the development of narcissistic traits to distinctness of preverbal body image, degree of 'disorganization' on recognizing the mirror image, and the tendency to field-independence and -dependence as noted by Machover. One may postulate that some

narcissistic neurotics are compensating for an indefinite body image by adornment, and seeking reassurance in the mirror and in approval from others.

The extension of the body image outward beyond the body was noted by Schilder, who referred to clothes as such an extension, and the relationship of the motor car to the ego is part of current folklore. Critchley has noted that parietal lobe lesions producing unilateral neglect of the body result also in neglect of adjacent inanimate objects, thereby disrupting dressing. Erikson (1958) has shown how the play constructions of boys and girls differ: those of boys emphasizing towers, elaborated closure, and channels of flow, while those of girls tend to be open and in a context of intrusion from outside. The difference may well be a reflexion of attitudes to and awareness of their own bodies. Reich (1933) in his concept of 'character armour', and the social self that protects against both frustrating reality and unconscious dangers, described interaction between the individual's personality conflicts, his expression of these in patterns of muscle tonus, and the repercussions of these tonus patterns upon his way of experiencing himself and others. Cutner (1953) has written of a method of treating neuroses by encouraging awareness of body tensions as analytical clues. Jung's notion of introversion as a withdrawal into the protective container of one's own body and personality would seem to be of relevance to the social implications of body image; and this in turn to the interesting sociological types of 'Inner-directed' and 'Other-directed' defined by Riesman *et al.* (1950), involving, respectively, an internalized set of goals, and sensitization to the expectancies and preferences of others.

A direct experimental approach

An impressive body of work has been built up by Seymour Fisher and Sidney E. Cleveland of Baylor University, relating body image to the life-style of normal individuals, patterns of family and group behaviour, physiological reaction patterns, site of cancer-

development and other psychosomatic correlates, and psychiatric disorders. Basic to their work, which is experimental and characterized by careful statistical control, are the Barrier and Penetration Scores derived from Rorschach records. Each response made by an individual in interpreting the Rorschach inkblots is examined, and is scored appropriately if it indicates a tendency of the individual to conceive of the boundary of his body as impenetrable, or as disrupted. Fairly definite criteria are laid down for assessment, and a high inter-tester reliability is claimed. Thus, for example, individuals with high Barrier Scores, as compared with low scorers, tend to show a high level of aspiration and self-gratification, take part in group activities, but show out-group tolerance and are less suggestible; they show greater sensitivity to light touch and greater muscle tension; they are more prone to develop externally situated cancers, and dermatitis and arthritis rather than gastro-intestinal disturbances.

In one study, Fisher & Cleveland (1958, p. 233) compared 40 cases of anxiety reaction, 40 undifferentiated cases of schizophrenia, 40 patients with paranoid schizophrenia, and 50 normal subjects. Penetration scores increased from normal to neurotic to psychotic subjects; the normal and neurotic groups gave similar Barrier Scores, together significantly higher than the psychotics. The Penetration responses of the schizophrenics were usually literal fantasies of bodily disruption or disintegration, again suggesting the inter-relationship of body image and ego. In another study (1958, p. 244), this time a longitudinal one, 25 cases of anxiety neurosis were used. A Rorschach record was obtained from each patient before a course of psychotherapy (lasting 6 months-2 years) was started, and a second record was obtained at, or near the end of treatment. Of the 19 cases who improved, 17 scored higher Barrier and/or lower Penetration Scores on the second record, the other 2 were unchanged; of the 6 who were no better, or worse, 4 had unchanged scores, 1 a higher Penetration Score, and the last one

had higher Penetration and Barrier Scores. The extension of this work, including extraverted as well as introverted neurotics (in Eysenck's terms), may be rewarding; as an investigation by the present author (1959) into personality correlations in Contact Dermatitis suggests that differences between introverts and extraverts may include appreciable differences in the Barrier Score.

Fisher and Cleveland point out that the Barrier and Penetration Scores show little relationship to the current structure or condition of the body. Thus there is no correlation with Sheldon's body-type classification, despite the cross-references he has made between physique and temperament; and disablement or mutilation, as by poliomyelitis or colostomy, does not appear to affect the scores. The scores would appear to be related to basic personality factors, probably arising early in development. Moreover, in a fascinating study (1958, p. 339), they demonstrate neurophysiological correlates of their body-image indices. On the basis of evidence linking a high proportion of α -wave activity on the electroencephalogram with passive reaction patterns, and a low proportion with active reaction patterns, they obtained Barrier Scores from 24 subjects with more than 50% α -activity for 34 with less than 50%. The difference in the median scores, respectively 3 and 5, was highly significant ($p=0.001$), indicating that a well-defined body image (high Barrier Score) may be associated with low α -activity, and a poorly defined one (low Barrier Score) with high α -activity. Moruzzi & Magoun (1949) have shown how stimuli which alert the organism act via the 'arousal system' of the reticular system and the diffuse thalamic projection to the cortex, where α -activity is decreased. Fisher and Cleveland's observation that subjects with high Barrier Scores have a high muscle tension is pertinent to this, as Gellhorn (1957) has pointed out that proprioceptive afferents play an essential role as activators of the arousal system. In addition, he has shown that proprioceptive afferents increase hypothalamic activity, and

that variations in this effect influence the pattern of emotional responses, e.g. with predominance of fight or flight reactions in animals. Because the effect is principally on the posterior (sympathetic) nuclei of the hypothalamus, variations would appear to affect autonomic balance. This in turn may affect perceptual mechanisms, as mentioned earlier (Callaway & Thompson, 1953). Much work has already been done relating autonomic balance to psychological illness (e.g. Funkenstein, 1951).

It would be interesting to relate Fisher & Cleveland's work to the Sedation Threshold of Shagass, which has been correlated with clinical ratings on a hysterical-obsessional continuum (Shagass & Kerenyi, 1958), and with the dimension of introversion-extraversion, thus linking it with the work on conditioning and perception of the Eysenck school. Shagass (1954) claims it is related to neurotic tension, which he suggests is due to overactivity or impaired regulatory function of the reticular 'arousal system'.

It might be worth integrating the study of autonomic and electroencephalographic patterns with the approach of Fisher & Cleveland, in different neurotic groups. Reaction to stress in neurotics and in children should be considered from the point of view of emotional patterns, and also intellectual functions, viewed developmentally. It may be that the imbalance between instinct and reality is accompanied by intellectual disturbance, such as regression from formal to intuitive thought (Piaget).

SUMMARY

The origins of the concept of body image lie in the study of gross disturbances resulting from organic brain lesions, particularly of the right parietal lobe (Critchley). However, apparently related disturbances occur in such transient disorders of cerebral function as epilepsy and migraine, and in psychoses. Depersonalization has been held to be both a body-image phenomenon and very frequent in neuroses (Schilder). In the identification of

the ego with body percepts (Freud), we are able to relate metapsychological theories of normal and neurotic personality development to a psychophysiological scheme. As the central representation of reality-testing organs and instinctual processes (Kubie), and as a basis of rationality (Meredith), the body image would be concerned with the adjustment of instinct to external reality. Disturbances of such adjustment, as seen in neurotic conflict, could thus be paralleled by body-image disturbances. These might be total, as in depersonalization, or partial, with only some systems affected, presumably determined during development. Evidence is quoted that suggests the influence of body image both on

perception and behaviour. Finally work is described (Fisher & Cleveland) that relates measured body-image indices to neurotic, psychotic, and normal categories, and to neurophysiological variables, directly, and by inference from other work (Shagass). It is suggested that the development of this work may prove of value in testing analytical hypotheses and extending fundamental knowledge of neurosis.

ACKNOWLEDGEMENTS

I have much pleasure in thanking Prof. G. R. Hargreaves for his encouragement and for reading the manuscript; and Miss Jean Stewart for her great help in typing it.

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INTEGRATION AND DIFFERENTIATION IN SCHIZOPHRENIA: AN OVER-ALL VIEW

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From a phenomenological viewpoint, schizophrenia can be seen to consist essentially in an impairment of both *integration* and *differentiation*, which, as I shall attempt to show, are but opposite faces of a unitary process. From a psychodynamic viewpoint as well, this malfunctioning of integration-differentiation seems pivotal to all the bewilderingly complex and varied manifestations of schizophrenia, and basic to the writings on schizophrenia by Bleuler (1950), Federn (1952), Sullivan (1947, 1953, 1956), Fromm-Reichmann (1950, 1952), Hill (1955), and other authorities in this field.

The term 'integration' is used here in an inclusive rather than highly limited sense; I shall discuss integration as a process which pervades multiple personality-levels and personality-areas. This paper is intended to be relevant to integration of the self-image, integration of one's experience of the surrounding object-world, integration of ideational content with affective impulses, and so on. The term 'differentiation' is employed here in a similar general sense and I am interested in differentiation as taking place in these same numerous areas of personality-structure and personality-functioning.

The term 'differentiation' is intended to bear the dual connotation conveyed, as I understand it, in common psychoanalytic and psychiatric usage. It connotes, as in biology, the elaboration of distinctive, specialized characteristics of structure and function as well as an ability to distinguish between, or to discriminate, fundamentally different objects and experiences. To take an example, this

dual connotation operates when we say that a relatively mature person is capable of achieving a highly differentiated conception or image of another person. This would imply both that he is relatively well able to grasp the highly complex, special ramifications of the personality of the other, and also to distinguish between that personality and other personalities with whom he has had experience.

Taking the matter of *integration* first, we find that when we assess the schizophrenic individual in terms of the three classical Freudian 'compartments' of the personality—id, ego, and superego—we discover these three personality-areas to be poorly integrated with one another. The id is experienced by the ego as an intensely inimical foreign body, which threatens to be overwhelming. In the more normal person it is rather a repository of primitive drives toward which one can maintain a basically friendly and receptive attitude, and which represents priceless well-springs of energy. The ego itself is severely split in the schizophrenic, as many writers have described, sometimes into innumerable islands which are not linked discernibly with one another. The superego, rather than being, as in the normal person, in the relation of a firm but friendly and helpful guide to the ego in the latter's efforts to cope with the id-impulses and with the outer world, stands in the nature of a cruel tyrant whose assaults upon the weak and unintegrated ego are, if anything, even more destructive to it than are the accessions of the threatening id-impulses (Szalita-Pemow, 1951). Moreover, the superego, even in itself, is not well integrated. Its utterances contain the most glaring of inconsistencies from one moment to the next, entirely comparable with a parent who

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is not only punitive but bewilderingly inconsistent. Jacobson (1954*a*, 1954*b*) has shown that there is actually a dissolution of the superego as an integrated structure, and a regressive transformation back into the threatening parental images whose conglomeration originally formed the superego.

With this structural state of things existing in the personality of the schizophrenic, it naturally follows that he functions in a poorly integrated fashion. When we perceive him in a temporal dimension, we find that he cannot integrate his life-experiences over a span of time as being all part of a continuing, unbroken pattern. Instead, his present and past experiences become all jumbled up, in the way which Federn (1952) has pointed out in his description of the phenomenon of regression to earlier ego-states. When we perceive him in the dimension of immediate interpersonal experience, we find that rather than his having an integrated over-all emotional orientation toward the other person, his reactions to the latter are, instead, an uncoordinated welter of ambivalent feelings—feelings which suddenly erupt, or as suddenly become unavailable to him through repression, in a fashion which severely limits the possibility of his developing a continued, integrated interpersonal relationship.

Differentiation is a process which is essential to integration, and vice versa. And when we look at this process of differentiation in the schizophrenic person, we find it to be similarly severely impaired. It is difficult or impossible for him to differentiate between himself and the outer world; his ego-boundaries are unstable and incomplete. He often cannot distinguish between memories and present perceptions. Memories experienced with hallucinatory vividness and immediacy are sensed as perceptions of present events, and perceptions of present events may be experienced as memories from the past (which may account for many instances in which schizophrenic patients speak of events in the immediate situation in the past tense). He may be unable to distinguish between emo-

tions and somatic sensations; feelings from the emotional sphere often come through to him as somatic sensations, or even as variations in his somatic structure (changes in the size, colour, and so forth, of bodily parts).

Hartmann (1939) made this interesting point in regard to the early development of psychic structure: '...there is no ego before the differentiation of ego and id, but in the same sense the id does not yet exist either. Both are products of a differentiation process.'

He has difficulty in differentiating, perceptually, one person from another, so that he is prone to misidentifying one person with another; part of this misidentifying often involves his experiencing transference-phenomena not only at an unconscious level, as does the neurotic person, but at a conscious level. He may *consciously* experience the therapist as being, for instance, his father or mother or brother. In the conduct of his daily life and in his communicating with other persons he is unable to distinguish between the symbolic and the concrete (Bateson, Jackson, Haley & Weakland 1956). If the therapist uses symbolic language the patient may experience this in literal terms. On the other hand the affairs of daily life (eating, clothing oneself, sleeping and so on), which we think of as literal and concrete, may be reacted to by the schizophrenic patient as possessing a unique symbolic significance which completely obscures their 'practical' importance in his life as a human being.

In this paper I shall make a general though necessarily brief survey of some of the aetiological factors at the basis of the schizophrenic's impairment of integration-and-differentiation, some of the manifestations of this impairment as shown in his ward-group relatedness and in the patient-therapist relationship, and in both these contexts I shall examine certain therapeutic measures, with particular reference to the ways in which they foster the resolution of this schizophrenic impairment.

Aetiology

This central difficulty in schizophrenia—the impairment of integration-differentiation—seems to be most fundamentally attributable to the schizophrenic's regression to the level of early infancy, at which developmental phase the infant had not yet become subjectively differentiated from the outer world; a phase in which he felt himself to be, instead, at one with all the world which came within his ken—a world much too vast for his rudimentary ego to integrate. This point has been conveyed by many writers—by Ferenczi in 1913 (Ferenczi, 1950), Nunberg in 1920 (Nunberg, 1948), Stürcke (1921), Federn at least as early as 1927 (Federn, 1952), Schilder (1935) (to mention only a few of these early investigators), and by many others in more recent years. One very recent work which illuminatingly traces this theme is the volume entitled, *Chronic Schizophrenia*, by Freeman, Cameron & McGhie (1958).

In the most general terms, normal psychological development consists in successive stages of personality-differentiation, each such stage being grounded in a newly won integration. Thus a reasonably healthy intra-uterine development is necessary to the earliest post-natal existence; a symbiotic relatedness between the infant and the mothering person is essential to the differentiation of the infant's own ego as distinct from the world about him; an effective level of non-verbal communication between infant and mother must be established before the beginnings of verbal communication can emerge in that relationship; and reasonably reliable verbal communication must be established, at a concrete level, between the child and those about him, before he can achieve the still more adult differentiation between concrete, literal thought and speech on the one hand, and abstract, metaphorical thought and speech on the other. In adulthood, similarly, we find that the better a person's integration, the more richly differentiated can his personality be—just as the tallest, most luxuriant tree is that with the most

extensive root system, embedding it firmly in the nourishing earth.

When we look, in terms of this developmental sequence, at the life-history of the schizophrenic, we find that many writers—including Reichard & Tillman (1950), Lidz & Lidz (1952), and Limentani (1956) to mention but a few—point to the existence of a symbiotic relatedness between the patient and his mother as being the most fundamental cause of his later schizophrenia. But Benedek (1949, 1952*a*, 1952*b*, 1957) has emphasized that a symbiotic relationship between infant and mother is *necessary* to the healthy development of the infant (as well as to the maturation of the mother herself). Mahler (1952) has found, in work with schizophrenic children, that the most deeply ill are those in whose relationship with their mother a symbiotic relatedness never became established; and my work with adult schizophrenics of various degrees of illness (1959) supports Mahler's observations.

What is pathological in this connexion, in the schizophrenic's life-history, is, as Mahler (1952) has stated, the circumstance that this symbiotic relatedness never became established, or became established but was not resolved in the normal way in early childhood.

Since the most significant traumata in the schizophrenic's life-history are generally conceded to be very early ones, I shall dwell upon the pathological circumstances obtaining in infancy and early childhood, and shall discuss further the relationship with the mother, before widening my focus to include aetiological factors in the family as a whole and, finally, in the over-all culture.

The mother is typically a deeply anxious, precariously integrated person whose chronic anxiety is probably intensified in her dealings with her young infant by reason of the symbiotic relatedness—with its concomitants of mutual incorporation (Searles, 1951) and poor definition of ego-boundaries—which the infant needs, and therefore strives to establish with her. Thus, in the relationship with the infant her own personality-disorganization is prob-

ably even greater than usual. One particularly poorly organized, changeable mother was described by her non-schizophrenic eldest son as typically coming home from the Baptist church on Sunday morning in a beatific state of religious exaltation, and the next moment furiously throwing a kitchen pot at one of the children. Her schizophrenic daughter insisted for years, in her therapy with me, that she had had not one, but many mothers, and once explained to me 'Whenever you use the word "mother", I see a whole parade of women, each one representing a different point of view'.

It is not surprising that some of these precariously integrated mothers shy away from entering into the necessary symbiosis with their young infants. And to the extent that they do enter into it, the infant's needs tend to be subordinated, as Hill (1955) has beautifully described, to the personality-needs of the mother. This has been considered by Bateson *et al.* (1956) as being a matter of the mother's hatefully holding the growing child fast in this symbiosis, through 'double-bind' injunctions, such that he feels damned if he does, and damned if he doesn't, say or do this or that. But my own work (Searles, 1958*b*) has shown that an even more powerful reason for the continuance of the symbiosis into the offspring's chronological adulthood, resides in his basically loving and loyal sacrifice of his own individuality in order to preserve the mother's unstable personality-equilibrium. He senses that his own sick personality-functioning dovetails with hers in such a way as to keep her head above water.

Particularly does the child bear the brunt of the mother's various massive dissociations, and from our own experience in therapy we know how alone and anxious it makes us feel to be aware of powerful emotions in the other person of which he himself is unaware. It took me several years to realize that, for example, the rape which schizophrenic women fear is, above all, rape by the mother who unconsciously phantasies herself as possessing a penis—a phantasied penis, that is, which is dissociated in the mother herself.

The mother, being a far from whole person, because of the dissociation of large elements of her personality, is unable to respond to the growing child as being a whole and separate individual. Thus we find her focusing unduly much upon certain anatomical parts or personality-attributes of her child, and blotting others out of her awareness—all of which greatly interferes with his developing a conception of himself as being a whole person. Hence, the adult schizophrenic patient may view himself, as one of my patients put it, as being 'a conglomeration of things'. Storch (1924) reports that one of his patients described himself similarly as being a 'conglomeration'.

The child's personality-fragmentation may be heightened, further, as a defence against the inordinately strong incorporative needs of the mothering person, whether this latter be the mother, the father, a nursemaid, or whoever. One patient for example felt for years in childhood as though the top of his head were gone. This incompleteness of body-image was eventually found, in psychotherapy, to be in part a reaction to the invasive curiosity of his nursemaid. It was as though the only sure way to keep her from reading his mind were for much of his psychological life to be barred even from his own awareness, and this, translated into corporeal terms, gave him to feel that the cranial area of his head was missing.

In broadening my focus to include the family as a whole, let me stress that the normal infant, and the to-be-schizophrenic child, is not able to differentiate himself fully from his environment, either from other human beings or from the inanimate objects in it (Searles, 1959). Thus we can understand how fragmenting it is to his developing personality for him to have, as often happens, a bewildering succession of mothering figures or many changes in residence, or for him to be exposed to intrafamilial relationships of marked dissension. He cannot incorporate usefully the personality ingredients of two parents whose interpersonal relationship with one another is a

very poorly integrated one—parents who seem (in some instances when one interviews both together) to function almost without any co-ordination at all, not even with an antagonistic one. Thus, at an intrapsychic level in the child, introjections from the mother cannot be well integrated with introjections from the father.

Further, as has been described by Wynne, Ryckoff, Day & Hirsch (1958), the actually great intrafamilial antagonisms are, by a kind of unspoken mutual pact, denied in the family, and a front presented of family oneness.

The paper by Wynne and his co-workers details, in an illuminating fashion, some of the means by which this family 'pseudo-mutuality' thwarts the child's differentiation—differentiation either of his personal identity or of his view of the world around him. In my experience, some of the families of schizophrenics make as great a fetish, by contrast, of what one might call pseudo-non-mutuality; Dr Joseph D. Lichtenberg emphasized this aspect of the matter upon hearing a preliminary draft of this paper.

One of my patients, for example, came eventually to tell me proudly of 'those crazy Randalls' (her parental family), and seemed to share in a general family pride concerning the chaotic discord which overtly permeated the family, and which overlay a very deep sense of family *coherency* that, in my patient at least, had been deeply repressed at the beginning of her psychotherapy.

It may well be that the kind of family background which I am detailing above, and which is described in other respects by Wynne *et al.*, is especially true for those patients in whom non-differentiation is the great problem; for those who show personality-fragmentation as the predominant problem, as did the woman just mentioned, this opposite kind of family mores may be more typical.

It should be noted that this tends to blur the real individual differences of the family-members, such that the child does not find, presented to him in the family, real acknowledged individuals with whom to identify. He grows up instead thinking of himself as simply a part of the supposed wholeness of the unitary family, undifferentiated from it. He grows

up assuming that it would be unpardonable disloyalty, and lack of love for 'the family', to differentiate himself from it. In such a family, the meaningful one-to-one interaction which any growing child needs with others in the family, cannot flourish and be acknowledged as such, for this is again tantamount to the two individuals' mutual disloyalty to 'the family'. Thus such interaction, which lacks consensual validation, tends to be dissociated, like the existing dissensions in the family which the patient tends to introject. In the latter regard, one of my schizophrenic women patients once gave me to realize that, in the midst of a furious upbraiding of me, she was misidentifying herself as her mother, and me as the mother's son. She thus beautifully revealed her prior introjection of the conflict between her mother and her brother.

If we keep in mind that, in all this, the growing child's ego is not fully differentiated from the personalities of those around him, we see that he dare not experience his feelings of *loss* as such feelings tend to arise, for this would be, as R. A. Cohen has pointed out (in a personal communication), equivalent to loss of his own ego. Thus the pattern is set which is followed in later life, of pathological guarding against loss: the patient cannot relinquish interpersonal relationships which are incompatible with one another, cannot face the loss of any of these relationships, because he dare not face the threat to his personal identity which the attendant separation-anxiety and grief would bring. Thus he moves, as he becomes adult, into a galaxy of interpersonal situations which are so incompatible among themselves, that *anyone* trying to lead such a way of life *must* be schizophrenic. For example, part of his personality may be invested in a marriage, another part in an extra-marital relationship with another woman, another part in a 'friendship' with the other woman's husband; or he may try to have two fundamentally incompatible relationships with one person. For example, he may be involved with a woman co-worker in a romantic relationship which is quite fully compartmentalized

from the co-worker relationship which the demands of their job require them to maintain.

What I am saying here applies especially to borderline patients. Among these persons too, one finds, not uncommonly, instances in which the patient is pulled apart, so to speak, by diverse career interests. He may be pursuing several quite different major career interests concomitantly, unable to relinquish any of them.

In the instances of more deeply ill schizophrenic patients, who are unable to form such contacts and career interests outside the family, we find, more frequently, that the patient is trying to develop and maintain fundamentally incompatible relatednesses with a family-member. This is most often, of course, with the mother or father, in whom he is trying to find, simultaneously, not only a parent but also a bosom friend, a spouse, a child, and so on. This latter kind of multifarious striving for diverse, basically incompatible kinds of relatedness is reproduced, in an intense form, in the transference-relationship to the therapist. Part of the therapist's function is now to help the patient to realize which of these strivings must be directed toward other objects; and to realize that *to a degree* these various strivings are *not* totally incompatible with one another—to realize that, at a feeling level though not in behavioural terms, loving relatedness means that each person is all persons and all things to the other.

These simplified examples illustrate what is really a much more complex pattern, in which one sees that the patient's ego is not merely *stretched* widely into diverse and mutually incompatible relationships, but that it functions as separate islands, and *must* function so in order for such a way of life to continue. And the 'relationships' to which I refer are not so much true interpersonal relationships as identifications with the other persons—investments of ego-identity in these diverse, mutually incompatible interpersonal situations, with a consequent lack of inner integrity, of continuity of identity and of the sense of being a whole person. Erikson (1956) has pointed out that normally this challenging task is met, and accomplished, in adolescence. He says that

'The final assembly of all the converging identity elements at the end of childhood (and

the abandonment of the divergent ones) appears to be a formidable task....

'...the adolescent, during the final stage of his identity formation, is apt to suffer more deeply than he ever did before (or ever will again) from a diffusion of roles....'

I have tried to show that the adolescent who goes on to become schizophrenic has had so little of continuous relatively unanxious well-integrated interpersonal experience, that he has not now the strength to face the separation-anxiety and grief entailed in the relinquishment of such incompatible (or as Erikson calls them 'divergent') identifications. He cannot make the necessary renunciations so that his ego can become consolidated into a whole, well-integrated, undiffuse one. Another way of putting it is to say that he cannot therefore develop an integrated way of life.

To broaden our focus concerning aetiology to include finally pathogenic elements in the culture, we should note that a culture in which more than one quarter of *all* hospital beds are occupied by schizophrenic patients* is a culture which presumably contains important schizophrenogenic elements, irrespective of the individual's particular family constellation. Fromm (1955) has described many such cultural elements in *The Sane Society*, which shows how various aspects of our technological culture foster in man a sense of alienation from himself, from his fellow man, and from the products of his work. Two of the consequences described by Fromm are what one might phrase as man's inability to become subjectively differentiated as a human being, and his inability to feel a sense of deep participation in the wholeness of collective man-

* This is based upon statistics collected in 1957, which show that slightly more than one out of every two hospital beds in the United States is occupied by a mentally ill patient, and that in New York State mental hospitals, for example, schizophrenics comprise about 57% of the patient population (National Committee Against Mental Illness, 1957).

kind. His existence is thus both fragmentary and poorly differentiated subjectively from the inanimate instruments and products of the technological culture which permeate his life.

Benedict (1938) has given a few extremely interesting examples among many possible ones, of the ways in which our particular culture imposes psychological discontinuities upon the developing child and adolescent, discontinuities which many so-called primitive societies do not intrude upon him. She notes, for example, that whereas in our culture one is expected to be obedient and submissive in childhood, and rather abruptly become quite oppositely assertive and dominant on reaching adulthood, many cultures do not expect any such opposite behaviours at these different developmental phases. She notes too, that many cultures do not require the marked shift in attitudes toward sexual behaviour which are required in our culture, as between the supposedly non-sexual boy or girl and the sexually competent man or woman. And by way of contrast to the attitudes toward productive work in our culture, where the individual is expected to contribute little in childhood and suddenly, much upon reaching adulthood, she describes how the Cheyenne culture met this aspect of development:

'... The gravity of a Cheyenne Indian family ceremoniously making a feast out of the little boy's first snowbird is at the furthest remove from our behavior. At birth the little boy was presented with a toy bow, and from the time he could run about serviceable bows suited to his stature were specially made for him by the man of the family. Animals and birds were taught him in a graded series beginning with those most easily taken, and as he brought in his first of each species his family duly made a feast out of it, accepting his contribution as gravely as the buffalo his father brought. When he finally killed a buffalo, it was only the final step of his childhood conditioning, not a new adult role with which his childhood experience had been at variance.'

Sociodynamics of the hospital ward

Turning from this brief discussion of the *aetiology* of the personality-fragmentation and non-differentiation which one sees in schizophrenia, let us now look at the kind of group-relatedness which the patient fosters, by reason of these symptoms, on the hospital ward. Many writings have dealt with this, and I shall mention only a few notable ones. Stanton & Schwartz's papers (1949 *a, b, c*) and their book *The Mental Hospital* (1954) describe how the patient's fragmentation increases upon his exposure to hospital personnel who are themselves involved in strongly felt, but not frankly expressed, differences of opinion concerning his management. Their writings understate the degree to which the patient's own personality-fragmenting anxiety fosters such dissension among staff personnel. An article by Perry & Shea (1957) makes up for this deficiency by showing the tremendously group-fragmenting effect upon the staff of one of the wards at the National Institutes of Health by an extraordinarily anxious and anxiety-provoking, personality-fragmented, man. And Main (1957), reporting in a similar vein, has shown how surprisingly strong were the dissensions among veteran nursing personnel concerning a number of such patients who had led stormy and therapeutically unsuccessful courses at his hospital in England.

Despite these and various other writings on this subject, however, there are a number of points which I have not found in the literature.

The kind of social situation which the ego-fragmented patient tends to foster on the ward can best be seen, I believe, as a process by which both differentiation, and subsequent integration of the disparate ego-fragments, must take place largely *external* to himself, in the persons of those about him, before these processes can be taken into himself. Of interest in this connexion is Sullivan's (1947, p. 10) view, concerning normal personality development, that 'The self may be said to

be made up of reflected appraisals', and Cooley's (1909) concept of 'looking-glass self'. I shall explain this process first in terms of the process of *integration*, since it is only this process upon which, to the best of my knowledge, existing literature touches.

Such a deeply disturbed ward-integration as Stanton & Schwartz, Perry & Shea, Main, and others, have described can be regarded as a kind of group-symbiosis, comparable with the infant-mother symbiosis but now embracing a group of persons, including the patient and the various members of the staff and fellow-patients involved with him. We see in this group situation precisely the same elements which have been described by Bowen (1956), Wynne, *et al.* (1958), Searles (1951) and other writers as characterizing the symbiosis between the mother and the to-be-schizophrenic child. It is a mutual entanglement, intensely felt and deeply ambivalent, wherein the other person is sensed as indispensable to one's own existence. The conflictual needs of each of the participants keeps the relationship in constant turmoil, and there is an over-all sense of maddening constriction.

In this group relatedness ego-boundaries are indistinct and the various participants function, as the anxiety in this group situation mounts, less in a truly interpersonal fashion than as a kind of unitary psychological organism. This may be crudely compared, in biology, to the human body in which the various different organs—brain, heart, liver, intestines, and so on—are indispensable to the maintenance of the over-all, unitary organism.

This means that the ward situation *looks*, at first glance, utterly unstable; but actually it represents a kind of social symbiosis which meets the neurotic, or psychotic, needs of the various participants sufficiently well so that it may endure for many weeks, and even for a considerable number of months.

Looking at it from the vantage point of the patient, each of the significant other persons in this group-symbiosis represents not only

a transference-figure, but also an externalized fragment of his own ego. In this sense, the complex, previously unsorted-out, unintegrated fragments of his own self become painted on the canvas which the ward situation presents to him. Thus this situation, anxiety-provoking though it is to all concerned, is a necessary preliminary to his *intra*-psychic differentiation and integration. Burnham (1956), in a valuable paper concerning misperception (misidentification) of other persons in schizophrenia, emphasizes that a patient's misperception of an aide (for example) as being a composite of figures from various areas of the patient's life, present and past, is a restitutive phenomenon—is indicative of his striving toward wholeness. Burnham likewise stresses the integrative aspects of a patient's misperceptions of the ward personnel collectively. We see over and over in individual therapy that a patient becomes aware of a previously-repressed fragment of his self only after seeing it first in a projected form, as being a part of the *therapist's* personality. This is the same process, occurring now in a group setting and with diverse projections operating simultaneously, from the patient to the diverse members of the staff.

The externalization goes on because the patient cannot as yet face the anxiety-laden realization that he has *within him* ego elements which are sharply conflictual. Instead, he unconsciously fosters, in the staff, diverse and conflictual views of himself. Instead of his becoming aware of the war within himself he fosters—largely unconsciously—the staff's warring with one another about him. To say that he is consciously 'playing them off', one against the other, is to grossly overestimate the degree of conscious control which he wields in the situation. It is more accurate to think of him as not yet being able to possess a whole ego within his own skin. His ego is only partial, a fragment, such that it is utterly necessary to him that others about him personify various of the other fragments; just as a heart must have lungs and a brain to go with it in order to survive. Parenthetically,

we know that somewhat less ill individuals, whose fragmentation is such that they are only schizoid rather than frankly schizophrenic, feel it very important in some instances that their friends be friends also with one another. This can be seen as the schizoid person's need that the externalized fragments of his ego, which are projected upon these various friends, be a continuous ego, rather than a split one.

From the staff's point of view also, this group-symbiosis, anxious though it is, meets neurotic needs. For those who are in a 'Good Mother' position, or who personify 'good' aspects of the patient's ego, there are the gratifications of feeling oneself to be a warmer, more loving, better human being than one's co-workers. For those in the 'Bad Mother' position, who represent 'bad' ego-aspects of the patient, there is the opportunity to ventilate pent-up resentments towards one's fellows, resentments which may long antedate the patient's arrival on the ward scene. There is also the opportunity to feel murderous in a relatively free and unalloyed way toward almost every one else in the situation (including the patient), without any irksome diluents of fondness, compassion, or comradeship. There is, I believe, a deep pleasure in our experiencing *any* feeling in a powerful and unconflictual way, and this social symbiosis is such as to minimize *intrapsychic* conflict for all participants—such as to provide for the externalization of each one's potentially *inner* conflicts.

Each member of the staff tends to relate himself particularly to a single one among the fragmented patient's various disparate personality-components—tends to see it as though this represented the totality of the patient. The nurse or attendant may assert that 'this is the way the patient *really* is', and feel that only she or he recognizes what the patient '*really* is like'. To the extent that they see the patient with this kind of tubular vision, one can surmise that such a relatedness is enabling this nurse to project, upon the patient, some unconscious emotion, or unconscious

self-image, which would cause internal conflict if it were recognized as an ingredient of the staff member's own personality. And I do not mean, of course, that they are 'only projecting'. Freud (1922) long ago pointed out that we project upon that which offers us some reality-basis for the projection. But it is presumably such a projective process which causes the various personnel members to have a personal stake in the group-symbiosis which these patients powerfully tend to foster on the ward.

To return to the vantage point of the patient, we should realize that he needs the 'Bad Mothers' as much as he needs the 'Good Mothers', for the preservation of both 'good' and 'bad' objects allows for the preservation of both his loving and his hateful feelings; these feelings being wedded, as I have indicated, to the objects. Both of these two broad categories of feeling are necessary to adult living. This simplified good-mother, bad-mother dichotomy is a fundamental step toward the infinite variation, or differentiation, of perceived objects and felt emotions that characterizes adulthood. If the staff all realized this, there might be a lesser tendency for them—for those in the 'good' as well as those in the 'bad' roles—to become involved in guilt, which in most instances hopelessly complicates this social picture and turns what could have been a step toward the patient's growth into an unworkable ward situation which ends in therapeutic failure and undermining of staff morale.

In any case, to the degree that the staff personnel go on, week after week, in such oversimplified, stereotyped roles, participants in this social symbiosis, they inevitably feel increasingly constricted. The healthy side of them demands something more than this symbiotic functioning as a less than whole person. One wants to function as, and be recognized as, a whole individual, even though this entails one's facing one's inner conflicts. Even the 'Good Mothers' among them encounter increasing frustration within themselves in the face of their constricting social

role, a role which forbids their having anything but love, solicitude and protectiveness toward the patient. Thus there develops a great deal of frustration-rage and consequent murderous feeling among the personnel, not only toward one another but toward the major fly in their ointment—the patient. But this intense rage tends—except, perhaps, in those who occupy the extreme ‘Bad Mother’ positions—to be maintained largely under repression, as being too threatening to the social structure and to each participant’s conception of himself.

I believe that some of the patient’s increasingly destructive symptomatology represents in such a situation an acting out of the repressed destructiveness which he senses to be at work in the personnel about him, analogous to the acting out, described by Johnson & Szurek (1952), by the disturbed child who is giving vicarious expression to the mother’s own repressed destructiveness. I believe that his typically mounting anxiety is to a degree based upon a realistic fear of the formidably great measure of murderousness which the other participants in the symbiosis come to feel toward him.

In the face of the increasingly intense conflictual feelings which permeate such a group-symbiosis, regression deepens, not only in the patient’s behaviour but in that of the staff members as well. Not only do his demands become more infantile, but the personnel’s mothering, good and bad, tends to assume more and more primitive forms. Just as he tends to become a suckling, demanding infant at the breast, they tend to almost literally offer him a breast, ‘good’ or ‘bad’ as the case may be, rather than providing more adult forms of mothering. There is a nice illustration of this point, although concerning a not-yet-hospitalized patient, in one of Knight’s (1953) articles on borderline psychosis. Here we see what I consider regression not only in the patient, who moves nearly into a literally sucking-at-the-breast role, but also in the therapist—a woman dean, who is a self-styled psychotherapist—who moves, concomitantly,

nearly to the position of trying to literally give suck to the patient:

‘...The dean was subject to increasingly demanding expressions of need from the student. Interviews could not be terminated, since the girl refused to leave, and sessions came to be held at the dean’s home in the evening and on weekends.

The student refused to see a psychiatrist or to go elsewhere for help, and the dean felt cornered. The girl demanded to use the dean’s car as proof of her love and trust, and this was granted. Then she began to request that she be permitted to stay overnight in the dean’s home, and when this was granted, that she sleep in the dean’s bed with her. At times she expressed irrational hatred of the dean and pounded her with her fists. At other times she wanted to be held on the dean’s lap and fondled, and this wish was granted also. No real limits to her regressive behavior were set until she expressed a strong wish to suckle the dean’s breasts. Here the dean drew the line....’

In my experience, frequent and informal meetings of the various staff members who are involved in the care of these patients are of inestimable value, for at least three reasons. First, they provide an arena for catharsis of some of the powerful feelings which have been engendered, so that the emotions in the overall social group I have described can be kept within manageable bounds of intensity, thus avoiding the total fragmentation of the group. Secondly, they help the various individual members to go on functioning *as* individuals in their work with the patient—as individuals who are relatively free to act upon their own particular feelings about the patient. Thus he is presented with genuine people of various sorts, with whom to identify constructively, rather than being faced with a staff group which is struggling to preserve some pseudo-harmony, some ostensible unanimity of attitudes toward him, in order to hide their sharply divergent true feelings about him. We see how similar this latter unconstructive ward situation is to the pseudo-unanimity so frequently found in the schizophrenic’s family. Thirdly, these personnel meetings enable, again as part of the same process, a genuine

collaborative integration to develop within the working group—an integration of different attitudes toward the patient. This is a process which, as I have previously indicated, must take place external to the patient before integration of his own diverse personality-fragments can occur *within* him, through constructive introjection of the well-integrated staff group which is working with him.

At the beginning of this paper I described integration and differentiation as being opposite faces of a unitary process. It is useful, however, to conceive of schizophrenic patients as showing a disjointedness between these two phenomena.

This point has been touched upon by Hartmann (1939) in a paper on ego psychology: 'Precocity of differentiation or relative retardation of synthesis may disrupt the balance of these two functions'. And Hartmann, Kris & Loewenstein (1946) noted that '...acceleration of certain integrative processes may become pathological'.

In the normal individual, integration and differentiation proceed simultaneously and in pace with each other as a relatively smoothly advancing unitary process of personality-growth. But one can think of the kind of schizophrenic patient I have just described as showing an imbalance between these two part-processes; an imbalance wherein differentiation has temporarily outdistanced their integration, with consequent fragmentation of personality-functioning, such as I have described. There is a resultant need for *integration* as the most pressing problem at the point in their development when we see them on the ward and in the therapeutic situation.

By contrast, there are other schizophrenic patients, still more deeply ill than those I have just discussed, whose emotions, attitudes, thinking-processes, and other personality-aspects are not yet sufficiently *differentiated*, with the result that they are even more helpless than the former type of patients in discerning, sorting out and communicating to others, the welter of undifferentiated thought-and-emotion which is within them. (Freeman *et al.*

1958), noting how relatively unvarying are their descriptions of the chronic schizophrenic patients whose therapy their book details, state, 'It is our contention that loss of individuality is a feature of the chronic patient in the refractory ward'.) These are the patients who are unable to express their thoughts or feelings because they quite literally are unaware of—do not 'have'—thoughts and feelings, most of the time. One of them let me know that he felt himself to be in a 'mist'; another, when I taxed her with failing to report any thoughts or feelings and asserted that I could clearly see, from her rapidly changing facial expressions, that there was a great deal going on in her, explained, 'My face thinks, Dr Searles, but I don't think'. And these are the patients who, in their ward-life and in their psychotherapeutic sessions, are mute and motionless most of the time, and who show, at most, only stereotyped, repetitive verbalizations or bodily movements. Finally, these are the patients toward whom the staff members tend to have a genuinely unanimous, stereotyped view—a view in which there is, in a sense, not *enough* of the kind of disharmony which is so overwhelmingly strong among these same personnel in reaction to the former type of patient. In other words, these patients tend to go on, month after month and year after year, in a social role on the ward which is as stereotyped, as un-rich and as unvarying, as is, evidently, their subjective intrapsychic experience. Here, the pressing need is for greater *differentiation* to occur.

For these patients it is well that they be exposed, over the months and years, to a relatively large procession of different staff members, even if this entails a rather rapid turnover of personnel. In my experience, these patients are less likely to become further differentiated than if they are taken care of by only the same few nurses and attendants, for such veteran personnel tend to stay in their rutlike, stereotyped view of the patient. More often, it is some newcomer who sees in the patient a bit of differentiation which is trying to

sprout, so to speak—an additional personality-facet emerging, and who therefore finds himself reacting to the patient in a way which is new and unstereotyped, thus bringing to the over-all staff attitude a welcome piece of differentiation. This is essential because just as for integration, the process must take place first externally to the patient, in the staff, before it can be established in the patient himself.

Often there is evidence that, all along, the 'new' potentiality in the patient has been evidencing itself but, being unnoticed by the personnel, being therefore not interpersonally shared between any one of them and the patient, it has persisted as a dissociated aspect of his personality, for that which does not gain consensual validation from one's fellows tends to be dissociated from one's own awareness. For example, at Chestnut Lodge we have had for many years a hebephrenic middle-aged man who is quite generally regarded, by the staff, as good old 'Georgie'. The fact that good old, fawning, obsequious, doglike Georgie not infrequently evidences a loud, blood-curdling sadistic laugh seems simply not to be noticed by those who are most involved in his care. They apparently similarly dissociate the fact that his so-stereotyped 'How's everything, Lovey?' is at times said in a tone as if he were disembowelling the person whom he is greeting. If a new attendant would take full note of this sadistic, hostile side of good old Georgie, and persistently relate himself to that personality aspect, I have no doubt that the staff group as a whole would grow to have a more differentiated reaction to the patient, and this presently dissociated sadism would eventually emerge, bit by bit, into his own awareness. The result would be that he would be a more differentiated (and, at the same time, better integrated) personality, both subjectively and as viewed in the ward social setting.

Not long ago, as part of my supervisory work here, I sat in for a half hour with a therapist, his female schizophrenic patient and the husband and mother of the patient. The

therapist and the patient had been meeting thus with these two relatives of hers (who were here on an extended visit) for several sessions, and I had heard from the therapist a lengthy description of how these meetings had been going. At the beginning of the session in which I participated, I was immediately struck with the patient's frequent rubbing of her genital area. This was a blatantly obvious act, and was done in a manner indicative of both sexual excitement and anxiety. I soon asked her about this and, with evident relief, she described the embarrassment this uncontrollable, repetitious act had long been causing her. After the session I learned from the therapist that this stereotyped act had characterized her behaviour in each of the preceding sessions, but he had omitted any mention of them in his lengthy description of these group-interviews. Until I began asking the patient about this, she and her relatives, as well as the therapist, had been functioning as though there were a silent, unanimous, pact whereby the group's abundant verbalizations would never refer to this particular behaviour of hers.

For ward staff as well as for the therapist, the highest order of skill consists, in this connexion, in the *timing* of the relating of oneself to the patient's dissociated process; in sensing *at what point* the patient is ready now to face this aspect of himself, or more accurately, in sensing at what point one's relationship with the patient is strong enough to warrant a mutual coming-to-grips with the dissociated material. Prior to such a time, it may be the ward-staff's or the therapist's unconscious intuition which causes one not to take notice of the dissociated processes in the patient; an intuition which tells one that it would be premature to try to integrate these, as yet, into one's relationship with him. But in my experience, errors are as often made on the side of postponing this intervention too long, or of never accomplishing it, as on the side of burdening the patient by a premature response to processes of which he is unaware in himself.

It is often the therapist who sees a new potentiality in the patient, a previously unnoted side of him which heralds a phase of increasing differentiation. And frequently the therapist is the only one who sees it. Even the patient does not see it as yet, except in a projected form, so that he perceives this as an attribute of the therapist. This situation can make the therapist feel very much alone and intensely threatened.

I have worked for several years with a hebephrenic man whom the staff had come to view, unanimously, as being a kind of pitiable, charming, little boy, endearing with his flashes of robin-like gaiety. He was regarded as basically hopeless because he had been hospitalized constantly, in a series of institutions, for sixteen years, was now in his mid-forties, and in nine years at the Lodge had shown relatively little change in the social position I have described. But as my work with him progressed, I began to have glimpses of murderousness—of precariously controlled violence—in him. The charge nurse and others of the ward staff did not see this, and the charge nurse even went so far, in her resentment of my jaundiced view of this robin, as to declare that if the patient ever did become assaultive, he would merely be acting out his therapist's own violence. This placed me in the position of not only being alone in my opinion, but in addition being held totally responsible for the patient's potential violence. And I would have looked in vain to the patient himself for any reassurance on this score, for he was meanwhile misidentifying me, in an acutely threatened fashion, as being 'Pretty Boy Floyd' (a boyish-faced gangster of the 1920's).

On the way to an informal staff conference concerning this situation, I fell downstairs and smashed my head into a wall with a much greater jolt than I ever received in four years of high-school football. It was such a jolt, in fact, that later in the day I obtained skull X-rays and a neurological examination which proved—I must for some reason add—to be negative. In any case I managed to go on to the conference, feeling marked indeed as a

violent man by the ward personnel who had witnessed this fall. But the conference, consisting predominantly in the expression of intensely antagonistic feelings between the charge nurse and myself, each documenting our stand with details from our respective work with this patient, proved to be the breaking up of what had been a long-frozen ward-attitude toward him, and I lived to see the happy day when the charge nurse herself became frightened at seeing the murderous side of this patient.

What I have just described touches upon the subject of the therapeutic relationship itself, and the severe stresses which the phenomena of schizophrenic disintegration and non-differentiation place upon the therapist as well as the patient.

The patient-therapist relationship

Focusing now upon the transference-relationship with the therapist, we find that the patient naturally brings into this relationship, just as he brings into the group-relatedness on the ward, the difficulties concerning differentiation and integration which were engendered by the pathological upbringing which I have described. And, as in the ward situation, we find that here too, advances in differentiation and integration necessarily occur first external to the patient—namely, in the *therapist's* increasingly well-differentiated and well-integrated view of, and consequently, response to, him—before these can become well established within him.

Because the schizophrenic patient did not experience, in his infancy, the establishment of, and later emergence from, a healthy symbiotic relatedness with his mother such as each human being needs for the formation of a healthy core in his personality-structure, in the evolution of the transference-relationship to his therapist he must eventually succeed in establishing such a mode of relatedness. In other communications (Searles, 1958b, 1959), I have described a few of the many clinical experiences which convinced me of this point.

This means that he must eventually regress, in the transference, to such a level in order to get a fresh start toward a healthier personality-differentiation and integration than he had achieved prior to entering therapy. This is not to say that he must act out the regressive needs in his daily life. To be sure, the schizophrenic patient, whether in therapy or not, inevitably does so to a considerable degree; but to the extent that these needs can be expressed in the transference-relationship, they need not seek expression, unconsciously, through acting out in daily life.

This symbiotic mode of relatedness is necessarily mutual, participated in by the therapist as well as the patient. Thus the therapist must come to experience not only the oceanic gratification, but also the anxiety involved in his sharing a symbiotic, subjective oneness with the schizophrenic patient. This relatedness, with its lack of felt ego-boundaries between the two participants, at times involves the kind of deep contentment, the kind of felt communion that needs no words, which characterizes a loving relatedness between mother and infant. But at other times it involves the therapist's feeling unable to experience himself as differentiated from the pathology ridden personality of the patient. He feels helplessly caught in the patient's deep ambivalence. He feels at one with the patient's hatred and despair and thwarted love, and at times he cannot differentiate between his own subjectively harmful effect upon the patient, and the illness with which the patient was afflicted when he, the therapist, first undertook to help him. Thus, at these anxiety ridden moments in the symbiotic phase, the therapist feels his own personality to be invaded by the patient's pathology, and feels his identity to be severely threatened, whereas in the more contented moments, part of the contentment resides in both participants enjoying a freedom from any concern with identity. In the latter connexion I recall a contented silence in which a schizophrenic woman and I, lounging in adjacent chairs, were listening to the borborygmal rumblings

in our abdomens. At one point, hearing some rumbling, she giggled and said she couldn't tell whether this came from her own 'stomach' or from mine, or from the first floor of the building, below. I wasn't sure either; but, above all, it didn't matter.

This same profound lack of differentiation may come to characterize, also, the patient's view of the persons about him, including his therapist; and at times, in line with his need to project a poorly differentiated conglomeration of 'bad' impulses, he may perceive the therapist as being but one head of a hydra-headed monster. The patient's lack of differentiation in this regard, prevailing for month after month of his charging the therapist with saying or doing various things which were actually said or done by others amongst the hospital staff, or by the family-members, can have a formidably eroding effect upon the therapist's sense of personal identity. But the patient may need to regress to just such a primitive, poorly differentiated view of the world in order to grow up again, psychologically, in a more healthy way this time.

Among the most significant steps in the maturation which occurs in successful psychotherapy are those moments when the therapist suddenly sees the patient in a new light. His image of the patient suddenly changes, because of the entry into his awareness of some potentiality in the patient which had not shown itself before. From now on, his response to the patient is a response to this new, enriched view of the latter, and through such responding he fosters the emergence, and further differentiation, of this new personality-area. This is another way of describing the process which Buber (in Friedman, 1955) calls 'making the other person present': seeing in the other person potentialities of which even he is not aware, and helping him, by responding to those potentialities, to realize them.

In my experience, schizophrenic patients' feelings start to become differentiated before they have found new and appropriate modes for expressing the new feelings. Thus a patient may use the same old stereotyped behaviour

or utterance to express nuances of new feeling. This is identical with the situation in those schizophrenics' families which are permeated with what Wynne *et al.* (1958) term 'pseudo-mutuality': 'In pseudo-mutuality emotional investment is directed more toward maintaining the *sense* of reciprocal fulfilment of expectations than toward accurately perceiving changing expectations. Thus, the new expectations are left unexplored, and the old expectations and roles, even though outgrown and inappropriate in one sense, continue to serve as the structure for the relation.'

The therapist, through hearing the new emotional connotation, the new meaning, in the stereotyped utterance and responding in accordance with the new connotation, fosters the emerging differentiation. Over the course of months, in therapy, he may find the same verbal stereotype employed in the expression of a whole gamut of newly emerged feelings. Thus, over a prolonged time-span, the therapist may give as many different responses to the gradually-differentiating patient as are simultaneously given by the various members of the ward-staff to the patient who shows the so-contrasting ego-fragmentation (or, in a loose manner of speaking, over-differentiation) such as I have described earlier in this paper.

Persistently stereotyped communications from the patient tend to bring from the therapist communications which, over a long period of time, become almost equally stereotyped. One can sometimes detect, in recordings played during supervisory hours, evidence that new emotional connotations are creeping into the patient's verbal stereotypes, and into the therapist's responsive verbal stereotypes, before *either* of the two participants has noticed this.

What the therapist does which assists the patient's differentiation often consists in his having the courage and honesty to *differ with* either the patient's expressed feelings or, often most valuably, with the social role into which the patient's sick behaviour tends to fix (transfix might be a more apt word) the therapist.

This may consist in his candid disagreement with some of the patient's strongly felt and long-voiced views, or in his flatly declining to try to feel 'sympathy'—such as one would be conventionally expected to feel—in response to behaviour which seems, at first glance, to express the most pitiable suffering but which the therapist is convinced primarily expresses sadism on the patient's part. Such courage to differ with the expected social role is what is needed from the therapist, in order to bring to a close the symbiotic phase of relatedness which has served, earlier, a necessary and productive function. Through asserting his individuality here and at many later moments in the therapeutic interaction, the therapist fosters the patient's own development of more complete and durable ego-boundaries. At the same time he offers the patient the opportunity to identify with a parent-figure who dares to be an individual—dares to be so in the face of pressures which are at times great; pressures from the working group of which he is a part, and from his own reproachful super-ego. What I am describing here is, of course, to only a minor degree a consciously planned and controlled therapeutic 'technique'. It is rather a natural flow of events in the transference-evolution, with which the therapist must have the spontaneity to go along.

Now let us see what transpires in the relationship between a therapist and a patient who is at the other end of the scale: one who is severely and overtly fragmented in his functioning, and whose urgent need at the moment is not for further differentiation, but for integration. Incidentally, we are dealing here not with two discretely different 'types' of patient but, perhaps more accurately, with markedly different phases of personality development. One finds that a single patient can show, over the course of time, each of these two extremes of, on the one hand, stereotypy with a need for differentiation, and on the other, fragmentation with a need for integration.

In the instance of the severely and openly fragmented patient, we find that this frag-

mentation places severe stress upon the therapist, fostering a sense of fragmentation in him as well. The therapist not only finds that, as one fragment of the patient's personality unpredictably replaces another quite different fragment on the scene of the therapeutic session, he, the therapist, finds his own responsive feelings as suddenly switching from, say, fury to compassion, or sexual titillation to loathing, or what-not. A patient who suddenly, while in the midst of delicately sensuous comments about the silken draperies in the room, switches to expressions of murderous feeling, or a patient who pauses in the midst of a savage paranoid castigating of the therapist to ask him, in a calm, friendly, matter of fact way, for a light for her cigarette, tends to make the therapist feel whipsawed between suddenly changing, intense affects within himself. And, worst of all, he sometimes finds himself having strong and utterly contrasting feelings toward the patient simultaneously. He may, for example, find himself reacting to the patient as being, simultaneously, a murderous woman and an appealing little child. The therapist's capacity to endure such a barrage of fragmentation-fostering experiences, from both without and within, is essential in his helping the patient to become better integrated through identification with the therapist whose personal integration can survive this onslaught.

Wexler (1952) and Hoedemaker (1955) have stressed the therapeutic value, in other connexions, of the schizophrenic patient's identifying with the therapist. I find that this holds also with regard to the integration of the previously disparate areas of his personality. I have regularly found that my own achievement of an integrated view of the patient, towards whom I have previously been responding upon two or more quite distinct and conflictual levels, is a prelude to the patient's own improved integration. It seems that the therapist needs to integrate, for example, his view of the patient as a murderous woman, with his view of the patient as an appealing child, and must respond to the patient as a

unitary person who possesses both these different personality-facets, before the patient can subjectively integrate these different, heretofore more or less dissociated areas into her conception of her self.

Processes in the patient's personality which exist in a dissociated state are maintained out of awareness, basically for the reason that the affects contained in them are so intense, so conflictual, and so opposed to those prevailing in the conscious ego. Early in the therapeutic relationship the therapist, even though able to discern in general terms the type or types of affects which are being expressed in the patient's dissociated behaviour or dissociated verbalizations, usually feels these dissociated personality-components to be strange, alien and bizarre. He does not yet find himself reacting to them with any fullness of personal response, for they are too much to one side of that interpersonal relationship which he shares with the more conscious areas of the patient's personality. But as the therapeutic relationship progresses and expands, this dissociated material gradually becomes turned toward the therapist, and he finds himself having, in response to it, feelings which grow concomitantly extremely intense. His freedom to express such personal responses—whether they be in the nature of fury, contempt, tenderness, erotic feelings (these last being no more than frankly acknowledged by him, when the patient shows a need for such corroboration), is of inestimable value in helping to integrate the erstwhile-dissociated components of the patient's personality.

Such integration has to take place first, or at any rate concomitantly, on an interpersonal level—namely, in the give-and-take of the therapeutic relationship—to the tune of intensely felt mutual expressions of affect, in order for the patient to become free from the dissociative compartmentalization, and to become thus a more whole person. In this process, that which was sensed by the therapist as alien and bizarre in the patient must come to be eventually 'taken personally' by the therapist. It must come to have the meaning

of a complex of very personal communications before it can lose its alienness, its dissociated nature, for the patient as well.

Paradoxically, the experienced therapist can thus predict, upon starting work with a patient, that the more queer, strange, alien, animal-like or otherwise not-quite-human the patient appears, the more deeply will the therapist's own most personal feelings eventually be plumbed in the course of successful therapy with this patient. He can predict that he himself will experience fury of an intensity he has experienced rarely if ever before within memory, fondness of a purity he has never felt perhaps since early childhood, compassion of a depth of which he had thought himself incapable. But he can predict, by the same token, that if the treatment is successful, he himself will emerge from it with a deepened regard for human beings and with an increased integration, in the phrase of Whitaker & Malone (1953), of his own 'multiple selves'.

To return to the kind of stress which he must endure along the way, we find that in some instances the patient's personality-fragmentation is so deep that even his body image is fragmented, and his view of the world regressed to a level at which other persons are viewed not as complete physical beings but as separate anatomical parts. Klein (1946) and Scott (1955) have postulated that the very young infant presumably views the mother in this way. The therapist, in this phase of the therapy, may find it so anxiety-provoking to find the patient expressing a conviction that his (the therapist's) head or hand, for example, is not his own, that he may resort to the somewhat relieving conviction that the patient is merely being hostile, rather than facing the anxiety-provoking fact that the patient is genuinely operating at so early a level of personality-development that even the patient's image of his own body, and of the bodies of other persons including the therapist, is not yet fully formed and integrated.

Further, in the transference, the patient persistently relates to the therapist as being a parent-who-has-many-dissociated-components-

of-personality, with the result that the therapist, in the face of the patient's persistently and convincingly relating thus to him, tends naturally to function accordingly, to a degree not usual for him in his general relationships with people. M. B. Cohen (1952) has made the valuable point that we inevitably respond to any patient's transference with functioning which, to a degree, is complementary to that transference. In the particular kind of situation which I am describing, the therapist tends naturally, in response to the patient's special kind of transference, to dissociate rage (for instance) which, in other kinds of transference-relationships with other patients, he might relatively easily admit into his own awareness. For example, to put it crudely but not inaccurately, if the patient reacts persistently and vigorously and long enough to the therapist as being a mother-who-has-intense-but-dissociated-murderousness, the therapist will in all probability come, one day, to find himself frightened at seeing how powerful are the murderous feelings which have grown up in him toward the patient.

Coleman (1956) and Coleman & Nelson (1957) have described a psychotherapeutic technique, which the former has employed with borderline patients, termed 'externalization of the toxic introject'. This technique consists in the therapist's deliberate impersonation of—conscious and calculated assumption of the role of—a traumatic parent, or other figure from the patient's early years, the long-standing introjection of whom has comprised a 'toxic introject', the core of the borderline schizophrenic illness. The authors' psychodynamic formulations are of much interest, and highly relevant to what I am saying above.

The great difference between Coleman's experience and mine, however, is that in my experience the therapist does not express, in such situations, affects which are merely a kind of play-acting, deliberately assumed and employed as a technical manoeuvre indicated at the moment. Rather, in my experience, the affects are genuine, spontaneous, and at times almost overwhelmingly intense. This differing experience presumably can be accounted for, in part, by the circumstance that whereas Coleman was dealing with borderline

patients, I have been dealing with frankly schizophrenic patients. With the latter kind of patient, because the patient's affects are so extremely intense and his ego-boundaries so very incomplete, his therapist's own feelings become more deeply and fully involved. I have discussed this subject at greater length elsewhere (1958*a*, 1959).

Both patient and therapist, in working toward the integration of the former's disparate personality-components (and, as Whitaker & Malone, 1953, have pointed out, this is a mutual process, involving the therapist's own personality-integration) have an inescapable resistance to overcome. Increasing integration involves, for both of them, loss. The integration of heretofore separate personality-fragments inevitably alters them—they lose, in the process, their pure-culture, pristine quality upon becoming adulterated, as it were, with other areas of the personality. And, correspondingly, one's own responsive reactions to them lose in purity.

I think for example of a woman who was, at the beginning of our work some years ago, extremely un-integrated in her personality-functioning. She behaved, from one session to the next, and often from one moment to the next, like a whole galaxy of utterly different persons. She has become, over the years, partly by dint of much hard work on the part of both of us, much better integrated. Life for her now involves more continuity, less anxiety, more genuine happiness; and I feel vastly more comfortable in the hours with her. But we have lost much, too. Just how much, I tend to forget until I look back through my old notes concerning our work. The beer-hall bouncer I used to know is no more. The captured American pilot, held prisoner by the Germans but striding proudly several paces ahead of the despised prison-camp guard, is no more. The frightening lioness has gone from her den. The incarnation of paranoid hatred, spewing hostility at the whole world, has mellowed into someone unrecognizably different. The endearing little girl can no longer hide the adult woman who is now part of her. The fresh-faced girl of sixteen or so

has come and gone—one sees her to a degree, of course, but alloyed now with other qualities. No more is there someone who tells me that I am a murderous woman who has killed my husband and am now about to kill my patient also. No longer is there someone, so far as I know, who thinks that I am a machine, sent to her room to destroy her. No one now perceives me as being, not a living person, but a pile of corpses, and so on. It is as though a whole gallery of portraits, some of them beautiful and some of them horrible, but all of them free from diluting imperfections, have been sacrificed in the formation of the single, far more complex and many sided portrait, the relatively well-integrated person who now exists.

What I have been saying ties in with Rosenfeld's (1950) comment that schizophrenic confusion involves an anxiety lest the destructive impulses and objects destroy the libidinal impulses and objects, and my (Searles, 1955) comment that the schizophrenic is equally afraid that the hostile side of his ambivalent feelings will be destroyed by the positive (libidinal) side.

Of interest in this connexion is a comment by Hartmann, Kris & Loewenstein (1949) regarding the normal infant: 'We start from the assumption of the existence of an undifferentiated phase of psychic structure. During this phase manifestations of both libido and aggression are frequently indistinguishable or difficult to distinguish.' Presumably the schizophrenic adult's fear lest he lose either his loving or his hateful feelings (or both) is based in *part* upon the threat of regression to such an undifferentiated phase.

The above discussion is a result of my discovering that, here again, the schizophrenic's—and the therapist's—anxiety is well founded. Something is indeed destroyed, and must be destroyed, in the process of mutual integration which takes place in the therapy. In the instance of the woman of whom I wrote above, for example, I can no longer love her and be charmed by her in quite the way I formerly could, *nor* can I hate, loathe and fear her with quite the intensity and 'purity' which used

to be not only possible, but necessary, in the progress of the therapeutic relationship. My capacity to love, hate, and so forth, in those wave-lengths is not really destroyed, but it may be a long time, if ever, before I find in my life occasions which call forth such emotions. Insofar as the therapy has been successful, it has in effect, for all practical purposes, destroyed them. And though she on occasion tries to recapture, for example, her old venom, it comes out only weakly now, like a nostalgic echo. (When Wexler (1951) wrote, '...I have been through two years of hell-fire and heaven with a schizophrenic patient...', he was feeling, I surmise, a sense of not only triumph but also nostalgia.)

What we have gained more than makes up for, of course, such losses; otherwise her integration could not have progressed so far. She now has feelings—in the realm of both healthy anger as well as deep and mature fondness—and complexities of feeling of which she was clearly incapable at the outset of our work, and the relationship between us has become emotionally enriched likewise. I have discussed at some length the losses which were involved in this over-all gain, because I have seen, not only in my own work but in that

of fellow-therapists, how often the patient's and therapist's mutual, unconscious *denial* of this element of loss causes an undue prolongation of the disintegrated state.

As regards the patient's advancing differentiation too, there is a similarly great resistance in both participants. Diligently though both are working toward the delineation of ego-boundaries in the relationship, so that the patient may become subjectively differentiated as a truly individual human being, both he and the therapist must endure a genuine and deep sense of loss in the process of this accomplishment. Both must relinquish, as I have described elsewhere (Searles, 1958*b*), the sense of oceanic contentment, felt omnipotence, and mother-infant adoration which are ingredients of the symbiotic relatedness.

ACKNOWLEDGEMENTS

My interest in the subject of differentiation has been fostered, in part, by stimulating remarks which I have heard in recent years from Drs Robert A. Cohen, David M. Rioch, and Otto A. Will. The research was supported by a grant to the Institute from the Ford Foundation.

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THE DEVELOPMENT OF CONCEPTS IN A GIRL AFTER DIETARY TREATMENT FOR PHENYLKETONURIA

By EVE LEWIS*

Introduction

Phenylketonuria is an inborn error of metabolism which is almost invariably accompanied by grave mental deficiency. It is probable that the children are normal at birth but progressive intoxication by phenylalanine causes mental deterioration. In a severely affected child this may reduce him by the time he is twelve months old to the status of an idiot. In a number of cases a diet low in phenylalanine reverses the process of mental decay. It is possible to watch the child gradually improving in general intelligence and to measure his progress by standardized tests.

A number of observations have been made upon such children, and in one report (Woolf *et al.* 1958) the characteristics of improvement have been summarized as follows:

'It is of interest to note that, as these children begin to progress on the diet, improvement in different kinds of activities is not uniform but tends to accentuate what may be a characteristic profile. The first and most striking observation, patent to everyone concerned, is the increased physical activity with the accompanying progress in locomotor development. In a few weeks or months, the child may learn to sit, creep, stand and walk. He also shows improved social alertness, enjoying the contact of other people and losing some of the apathy and general mental dullness previously observed. Progress in speech and also in hand and eye co-ordination is frequently much slower, and manual skill may be delayed for some time. The number of cases studied in this way so far is too few for any statistical evaluation of a characteristic profile to be

valid, but it appears that the child, still limited in general mental capacity, tends to concentrate on one type of activity at a time, locomotor improvement being at first most strikingly present in the majority of cases.'

In the present study, whilst the usual psychometric tests were periodically administered, special observation was also made of the way in which the child's ability to form concepts developed. Such work is necessarily rather subjective. But the child, though mentally impaired, was physically stronger and more precise in her reactions than an infant whose life and mental ages would correspond to her mental age when the examinations were carried out. Consequently very clear and unambiguous response or lack of response could be discerned. Progress was also more rapid as the dietary treatment took effect, the child's overall mental age rising from 8 months 3 weeks to 2 years 7 months in the course of 14 months.

Reference was made throughout to Piaget's (1955) observations and concepts in his postulated order during the period of what he terms sensori-motor intelligence. His view is summarized thus by Berlyne (1957).

During his first two years, the child gradually advances towards the highest degree of intelligence that is possible without language and other symbolic functions. He begins life with innate reflexes, but these are, from the start, modified and made more effective by learning. New responses are soon acquired, and then complex solutions to problems are achieved by piecing together familiar responses in novel combinations. By the end of the second year, the first signs of the human capacity for symbolizations appear; he invents new patterns of behaviour which show him to be representing the results of his actions before they

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occur. In short the sensori-motor period sees attainments comparable to the highest found in sub-human animals.

This growing ingenuity in the face of practical problems goes hand in hand with a less 'ego-centric' and more 'objective' conception of the world. For some weeks after birth, the world must consist of a succession of visual patterns, sound and other sensations. The infant comes naturally to pay attention to these external events which are associated with satisfaction or which are brought about by his own actions. Gradually he builds up a view of the world as a collection of objects continuing to exist even when they are out of his sight and generally preserving the same sizes and shapes, despite the changes in their appearance that come with changes in position. Whereas no distinction between himself and what is outside him can have any meaning for him at first, he comes to conceive of himself as one object among the many that people the world, most of them unaffected by his activities.

The concept of an *object* is bound up with objective notions of *space*, *time* and *causality*, which the child does not possess as part of his native endowment but has to build up gradually through interaction with the world. After learning to select appropriate spatial directions and temporal successions for his actions, he comes to respond to the positions and times of occurrence of events outside himself, using his own body and his own actions as reference points. Finally he conceives of a space and a time in which both he himself and external objects are located. He learns, for example, to distinguish occasions when objects are moving independently of him from occasions when objects are merely changing position because he is moving among them. Similarly he progresses from an understanding of the relationship between his responses and the consequences to an understanding of the causal influence inanimate objects can exert on another and even on him.

Piaget distinguishes six stages during the first two years of life in the course of which the child constructs his world. Very gradually the infant comes to perceive the world as a place in which objects continue to exist even when he does not see them, preserving their qualities of shape, size, weight and so forth, though invisible or presenting different appearances

according to the position from which he views them. The stages are:

Stages I and II (c. birth to 6 months). The child makes no response when an object is removed from his perceptual field.

Stage III (c. 6 months to 10 months). The child continues to look for a time at the place where an object vanished from his sight. He can now make visual accommodation to rapid movements and endows objects with a certain permanence in so far as he continues the process of watching begun when they were present.

Stage IV (c. 10 months to 12 months). The child actively searches for an object which has gone out of his sight. But he regards it as having an absolute position, since he looks only in the place where he first saw it. He does not look for it in the place whence it disappeared.

Stage V (c. 12 months to 18 months). The child has a dawning capacity to take account of change of position, and searches for an object in the place where it was last seen, though it may be quite different from that in which it was originally seen.

Stage VI (c. 18 months to 24 months). The child searches in a sustained way, proving he is now aware that the vanished object still exists.

History prior to diagnosis

Mary C. was born in March 1955 of parents who are of average intelligence. She weighed 7 lb. at birth, which was normal. She was reported to be sitting up at 7 months and walking with assistance at 18 months. There was a normal eruption of teeth and some bowel control, though none of urine. Feeding was from cup or spoon; no attempt to pick up food was made. The mother reported her as saying Mama and Dada, but this was probably a hopeful interpretation of normal babbling. At 18 months Mary broke a leg. After this she made no attempt either to crawl or walk. Phenylketonuria was diagnosed in July 1957, Mary then being 2 years 3 months 3 weeks old.

*General picture prior to commencement
of dietary treatment*

Mary's overall mental age was 8 months, but she had reached different stages in different kinds of activity. She could sit erect on the floor and could progress by sliding in this position. She stood when held up. She was friendly to strangers and seemed to distinguish between them and her mother and the hospital staff, though she appeared to react to voice and touch rather than to sight. She could take off hat and shoes. She was responsive to sound generally and appeared to like wireless music. Whereas she had babbled in early infancy she now made few sounds other than laughing or crying, though at intervals she uttered single loud screams. Eye and hand co-ordination were good as regards grasping and manipulating objects, though prehension was rudimentary with no specialization of thumb and forefinger. Moving objects attracted her attention as long as they were in her visual field. She turned her head, but not the torso, to watch them. Anything presented to her was immediately conveyed to her mouth. She liked to tear up paper and to hammer any object she could grasp upon her bed-table. If a toy were placed out of her reach she leaned towards it, but did not try to move in its direction.

The most striking thing was that quite obviously no one object had greater meaning for her than another. Each was merely something to grasp, suck, or hammer upon the table. Although the volume of noise she made seemed to please her, she did not perceive that a wooden cube was more satisfactory in this respect than a small rubber doll. If she were banging with the doll she did not exchange it for the cube, with which she had previously been hammering, when this was again placed within her reach. It was particularly noticeable that when an object was slowly withdrawn from her field of vision, she did not even continue to look at the point where it vanished. Far less did she search for it when it was concealed, as she watched at

the foot of her cot, though physically she was capable of sliding in a sitting position towards it. At this stage Mary recognized in a rudimentary way certain people and objects, when they were present before her eyes, but it would appear that no image of them remained once they were removed. Their presence evoked certain motor patterns, which were well defined on account of her muscular development, but they had no permanence for her. Her universe was not beginning to be, as Piaget puts it, 'cut up into objects, into things conceived as permanent, substantial, external to the self, and firm in existence even though they do not directly affect perception'.

Progress towards object concepts

The first improvement noted, once dietary treatment had started, was that Mary became quieter. Hitherto she had, when not attended by an adult, rocked continuously or endlessly lifted the hem of her frock up and down before her face. She began to watch moving people or objects more attentively, turning both head and torso. When anything disappeared from her sight, she would continue to look for a brief space at the place where it had last been seen, appearing to await its return. One would surmise that people and objects were beginning to have form for her, and that some kind of discrimination was going on, as long as things remained visible. On one occasion the doctor who normally treated her stood for some ten minutes at the foot of the cot, while Mary was being examined. She then had to leave and Mary watched her go through the door to the right of the cot. During the following two minutes the child repeatedly looked at the position where the doctor had stood, and one had the strongest impression that she expected to see her there again. This would imply that some image of the absent figure remained for a short while in the child's mind. It was at this stage that one could note something which it is difficult to convey—namely expression in her eyes. When she was first seen they were expressionless and blank. Even when she smiled her

eyes did not seem to do so. She had not hitherto examined anything she handled, appearing to grasp by reflex action and to be satisfied by the motor pattern which was activated. Now she began to look at people and objects and one can only suggest that they were beginning to have some kind of meaning for her. Here again the fact that her features were more formed and her gestures more precise than those of a younger child of the same mental age gave a definite impression. It was as if she thought in a simple way about what she was seeing, and assimilated something of its content.

By the time the mental age had risen to 1 year 5 months 1 week, seven months after dietary treatment had begun, the general picture of Mary's mental ability was as follows.

Locomotor ability had improved by only two weeks. Speech was at the same point as when she was first examined. It must be assumed that early immobilization with a broken leg was partly responsible for there being as yet no effort to pull herself erect, to crawl, or to walk. It is also likely that the effort to speak was not made as she was in hospital and thus cut off from the regular talk which an infant hears in the home. On the other hand speech was beginning to have meaning for her. She knew her own name; desisted from an activity when one said 'No'; she handed objects when asked with an outstretched hand accompanying the request, and she put cubes in a box upon verbal encouragement and some demonstration. Now she quite definitely recognized people by their appearance and looked into their faces with an earnest questioning gaze when they spoke to her. She made a fairly systematic search for a hidden object and would remove two, but not more, covers to find it. If someone withdrew from her sight, she would wriggle round to watch the door or screen where they had disappeared. *Preference for the right hand was established. Prehension was fine and she imitated clapping hands.* By this time a younger brother had been found also to have phenylketonuria and was in the same hospital.

When he cried Mary was often placed in his cot where she would try to comfort him by patting and kneading his stomach. She laughed if she succeeded in making him laugh.

In the specific field of object-concepts she had arrived at a stage in which she could keep in mind a complete picture of the movements of a person or object in space, and could realize that they can be displaced in relation to one another. From her ready recognition of familiar people and objects it would appear that some image of them persisted when they were not present.

Three months later Mary could solve simple problems. She obtained objects placed out of her reach by pulling the paper on which they were lying, or a string attached to them. She replaced the square and the circle in a three-hole form board. She placed the triangle crosswise, but continued to press it firmly. It was quite evident that she had an image of its ultimately fitting into a cavity. When she was seated on the floor and a ball was carried by a devious route to a place where she could see it again, she at once slid by the shortest possible route to recover it. But when it was placed on the side of a low table farther from her, she did not slide either under or round the table when she found she could not reach it from the front. She had not yet arrived at Piaget's sixth stage, in which she could locate herself in space and represent to herself purposive displacements of her own body.

At this point Mary was discharged from the hospital and went home. When she returned two months later for a routine check-up and examination a considerable development in all directions except speech was apparent. Her world was now stable and consistent. She moved purposefully amongst the objects surrounding her, could remember previous positions, and was able to represent to herself the results of a course of action before embarking upon it. When a toy was *placed upon the back of a high chair, and a low table interposed between her and the chair,* she immediately crept under the table,

losing sight of the toy as she did so, stood up again and walked round to the back of the chair where, by stretching, she could just grasp the doll. Later the same day Mary's mother, carrying the younger brother, left the ward by one of its three doors, through which, incidentally, the child had never seen her pass before, as it led to an inner part of the hospital. The child displayed much anxiety, but was reassured when told that her mother was not going away and leaving her behind. Evidently therefore some memory of this having happened was in her mind. After an interval of fifteen minutes, during which she had played with another child, Mary was told to 'find Mummy'. She walked unhesitatingly to the correct door and opened it. Examined by the Terman-Merrill Scale, Form L, she achieved a mental age of 2 years 7 months. The Form Board gave her particular pleasure. She wanted to do it again and correctly inserted the circle and square, when it was rotated. (This is recorded because, three months later, when part of a progress film was being made, she was again presented with the Form Board. She recognized it before it was at rest on the table in front of her, snatched out the three insets, jumbled them in her hand and then replaced them correctly, laughing delightedly to her mother as she did so.)

At about this time her mother thought that the child might be dreaming. She sometimes awoke crying and looked delighted when her mother came to her. Since she still uses only about twenty words it is naturally impossible to find out whether she was in fact dreaming. But her evident relief at the sight of her mother does suggest that, in sleep, she had become anxious at the thought of losing her mother. A somewhat more reliable fact is that she at times laughed aloud in her sleep, to quote her mother: 'in the way she does when romping with Carol' (her infant sister).

The most interesting and significant development noted at this examination was the intense imitative activity Mary now displayed. At this stage she reproduced as nearly as possible actions she had just perceived or

repeatedly perceived, applying them to the original object. Thus she powdered the baby, held her feeding bottle, tried to lift her if she cried, shook hands after her mother had done so, laughed when she did and so forth. She also tried on her mother's shoes, gloves and brassiere, and applied lipstick and powder to her own face. There was some 'deferred imitation' as Piaget has defined it. For example, on seeing the examiner take off her gloves, she picked them up and put them on her own hands, as she did with her mother's gloves.

Two months later Mary was visited and observed in her own home. By this time imitation had turned into creative play, the actions being detached from their initial stimuli. She now played with two dolls, in which she had previously shown little interest, her attention having been focused upon her sister and on what her mother did with her. She dressed, undressed, washed, powdered and fed the dolls with absorbed pleasure. The images of these observed activities had become symbols which she could manipulate at will in a game of make-believe. One could suppose that she had some understanding of the relation between the symbol and that for which it stood. She showed concern when her sister fell, but none when a doll was made to fall; but this may have been because the human baby cried lustily. An effort was made to find out if she discriminated between pictures of animals and living animals with which she was familiar. She stroked pictures of a cat, a chicken, a horse and a cow, this being taken to indicate that she knew they signified animals. But when given some grain, she looked to the back door, outside which chickens were kept, as if she knew it belonged to them and not to the pictured creature.

The mother reported a recent development in play which had puzzled and even alarmed her. She said that at times the child 'seemed to be playing with nothing'. She babbled to herself, gesticulated, smiled and appeared to see something or someone that wasn't there. Since Mary gives ample evidence of being

adjusted to reality, and since this behaviour was spontaneously described by her mother, it may be assumed that this is a beginning of some sort of wholly imaginative play. Her mother described a recent example in the course of which Mary had repeatedly pointed with excited laughter to something which appeared to be moving about the room. She seemed to follow its progress clearly and tugged at her mother to watch it also. On other occasions the mother had the impression that Mary's babbling and laughter were a kind of interchange between her and what she was seeing. It is legitimate to infer that the child is now experiencing eidetic images. Possibly there is an imaginary companion of the kind which some children develop between the ages of two and four. If this is so it would mark a further stage in her development away from the condition of near-idiotcy in which she was before dietary treatment began. When a child has reached this phase of mental growth he is no longer dependent for activation upon immediate and concrete stimuli. Now he is able to conjure up from his store of internalized and remembered symbols those images which are necessary for the play he has in mind, and he can mentally manipulate them to serve his purpose. He is not limited by the realism of fact, but can create a phantasy world of his own. In this world, thought is free to develop until it can finally become abstract and thus completely independent of sensory context. Allport (1924) has drawn attention to the profound importance of this stage of mental development for the child. It is one in which he 'studies out' the various possibilities for response in a stimulus situation, taking his own time and reacting accord-

ing to his psychological type. This experimentation in play with his images allows him to vary both their behaviour and his responses to it. By such mental processes he can come to an understanding of a situation and can think out its many implications for his own future behaviour in like or allied situations. It is a step towards the socialization of his thought.

Conclusions

Under dietary treatment this child passed through the sensori-motor stages of development as postulated by Piaget. She progressed from a world in which the visual patterns, sound and other sensations which reached her had no meaning and no permanence, to one in which she could envisage herself as placed, together with the clearly defined objects of this world, in time and space. From this she has further progressed (April 1959) to a stage where she has built up a store of images which she can use as symbols in the course of imaginative play. Through such play she is learning how to think in the abstract.

ACKNOWLEDGEMENTS

I wish to express my most grateful thanks to Dr F. S. W. Brimblecome and Dr Margaret Stoneman for the opportunity to study Mary, and for the help they have given me in compiling this record of her mental development under their treatment. I am also greatly indebted to the staff of the children's ward in which Mary was, and to her mother. They were all most helpful in supplementing my personal observations and in giving me ready access to the child.

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LANGUAGE IMPAIRMENT IN A GROUP OF SCHIZOPHRENICS*

GEORGE SETH AND HALLA BELOFF

Schizophrenic dysfunction in the sphere of communication in general, and in the means of communication, language, in particular has been the object of much study and much writing. Observers differ, however, in the interpretation of this dysfunction. Cameron refers to social disarticulation; Kraepelin spoke of disconnexion of thought; Goldstein interprets the disorder in terms of impairment, if not loss, of abstract behaviour; Hanfmann and Kasanin refer to a loss of generalizing ability. Shakow notes the inability to maintain a set, and Bateson uses the concept of the Double Bind.

In so far as any of these positions can be objectively stated and have been tested, contradictions appear. A review of the literature would indicate that at some time all the following conclusions have been drawn: (i) that schizophrenics show no particular verbal impairment (Babcock, 1930); (ii) that they do show verbal impairment (Moran, Moran & Blake, 1952); (iii) that they show impairment only at the higher levels of verbal functioning (Feifel, 1949); and (iv) that they show impairment of every ability, including the verbal (Binder, 1956).

Some of this confusion seems to have arisen, not because of the nature of the phenomena as such, but rather because relevant concomitant variables have not been specified clearly enough. Comparisons have been made between data that deal certainly, both with schizophrenics and with language, but on finer points are not comparable. Three of the facts to which insufficient attention has been paid are: the severity of the schizophrenic

process in the experimental group, the precise comparability of the control group, and most importantly, the level of language performance tested. The present project was designed to specify each of these variables in some detail.

The status of the schizophrenic group

Usually it is possible to know only that the experimental group is disturbed enough to be hospitalized and yet sufficiently 'in contact' to be able to complete the testing procedure. We hoped to provide more information.

In general, our group consisted of ten male and ten female schizophrenics selected on the basis of psychiatric diagnosis (and having no intercurrent affection). No further breakdown into subtypes of schizophrenia was made. All the subjects were under the age of forty, and none had been hospitalized for the first time more than five years earlier. Of course, they again had to be able to understand what we wanted them to do.

But more particularly, each subject (control as well as experimental), completed three objective personality tests, the three tests that distinguished most clearly between schizophrenics and normals in Eysenck's 1952 study. These are: Size Estimation (drawing half-crown from memory); Reading Speed (being the time taken to read a short prose passage); and Length Estimation (reproducing lengths of 2ft., 1ft., and 8 in. from memory). From the scores of these tests it was hoped that the degree of pathology could be more precisely established. The results in Table 1 show that all the differences are in the predicted direction. Although our schizophrenic means are similar to those reported by Eysenck (1952), the control means tend to approach them, and variances are relatively high. The differences are not uniformly significant, and hence

* Based on a paper to the Annual General Meeting of the British Psychological Society, April 1959. Dr Seth is Professor of Psychology, Queen's University, Belfast, and Dr Beloff is Research Assistant. Manuscript received 9 July 1959.

the tests do not serve our need for fine discrimination of the experimental group. However, it may be considered that the data from the Wechsler-Bellevue Scale adds some information on status.

Selection of the control group

Our group control was selected from the patients in two tuberculosis hospitals serving the same geographical and socio-economic area as the mental hospital.

Table 1

	Schizophrenic		Control			
	Mean	S.D.	Mean	S.D.	<i>t</i>	<i>P</i>
<i>Eysenck personality tests</i>						
Size estimation						
(a) Maximum diameter	36.2	11.4	35.0	5.9	0.59	—*
(b) Error	9.6	7.5	5.5	3.6	2.33	0.02
Reading speed	25.4	6.0	20.8	4.2	3.16	0.01
Length estimation						
(a) Overestimation	32.7	49.0	20.2	30.6	0.76	—
(b) Underestimation	14.4	22.1	10.6	12.8	0.21	—
(c) Error in 8 inches	6.5	7.1	4.5	3.6	1.45	—
(d) Error in 1 ft.	12.2	13.1	9.8	19.0	0.38	—
(e) Error in 2 ft.	28.1	31.4	17.5	19.3	1.33	—
(f) Total error	46.8	44.6	30.5	40.3	1.20	—
<i>Wechsler-Bellevue verbal scale</i>						
Verbal I.Q.	90.2	11.8	96.4	14.9	2.78	0.02†
Information	6.6	2.4	8.2	2.9	3.93	0.001
Comprehension	7.6	3.3	8.6	3.6	1.75	—
Arithmetic	8.0	3.1	9.2	3.5	2.07	—
Digit span	7.6	2.8	8.4	3.4	1.37	—
Similarities	7.8	2.7	8.6	2.8	2.12	0.05
<i>Moran verbal ability test</i>						
Word association						
Relatedness score	37.9	12.7	53.4	9.7	5.86	0.001*
Synonym recall						
Number correct	17.8	17.7	24.0	10.7	4.19	0.001
% correct	50.0	23.1	73.0	11.8	4.85	0.001
Sentence construction						
Number adequate	6.6	2.3	9.9	1.9	4.31	0.001
Similarities						
Adequate score	5.0	3.2	9.7	3.0	7.06	0.001
Abstract score	3.0	3.0	6.2	3.5	4.93	0.001
% Abstract/Adequate	48.8	37.3	60.2	19.5	1.76	0.05
Analogies						
Number correct	4.6	2.8	7.1	1.9	4.13	0.001
<i>Type token ratio</i>						
Mean segmental TTR	55.6	3.8	58.9	1.6	3.98	0.001*
Time	384	92.5	304	31.0	3.59	0.01

* One-tailed test.

† Two-tailed test.

Six criteria for matching individual pairs were used:

1. Sex.
2. Age—both groups have a mean of 33 years; 4 years is the greatest difference within any one pair and the mean difference is 2 years.
3. Number of years of schooling—none of the subjects had had any period of higher education.
4. Occupation—both members of each pair fell into the same Census Social Category.
5. Wechsler-Bellevue Vocabulary raw score—the mean score of the experimental group is 16 points, and the mean of the control $16\frac{1}{2}$, the mean difference between pairs being less than $\frac{1}{2}$ point. (It is, of course, this matching on verbal base-line that makes the rest of the language ability comparisons meaningful.)
6. Length of hospitalization—Arieti (1955) has suggested that stay in an institution *per se* might affect language performance. Particularly we felt that it might produce a lack of variety and fluency. Therefore we arranged that the control subjects should come from another *hospital* and that pairs should be matched for length of stay as far as possible. This could not be done completely satisfactorily; on the average, 'time away from the outside world' for the schizophrenic group is 14 months and 9 months for the control.

The level of language performance tested

Cronbach (1942) has written that orthodox vocabulary tests sample too small an area of verbal functioning. Different types of verbal performance are not perfectly correlated and need to be investigated individually. To quote: 'There are two extremes of knowledge—a word as symbol that can be put to *some* use in thinking and communication, and the word as representing the total fullness of meaning which human experiences may associate with it. Between the two extremes are naturally all degrees of meaning for different individuals.' He envisages variation in, for example, precision, degree of generalization, application and, of course, availability.

This is the crucial point: different types of verbal functioning are not perfectly correlated in a normal population. Where decrement is under discussion, this is obviously a still more important point. The consideration of levels

might, it appears, bring some order into previous findings. Schizophrenia may result in the impairment of some levels of functioning, and yet leave others intact. This then might account for the fact that some investigators have found gross impairment, while others have found little or none.

The point of the present project was to investigate as many levels as was reasonably possible, and to compare not only the differences between the groups *at* each level, but to compare also differences *between* levels. Thus a multi-level analysis comparing matched schizophrenic and normal groups should provide a rigorous test of the hypothesis that in schizophrenia significant language dysfunction is present which goes undetected in the usual tests. To this end we have used the verbal battery described by Moran (1953), replicated part of his procedure and extended his work, downwards as it were, by including a measure of simple 'availability' of words.

The procedure and the tests

The testing procedure, which was of course individual, went as follows: with the experimental group one of the investigators administered the Moran tests, and obtained a sample of the subjects' spontaneous language, to provide the availability measure, by engaging them in a conversation that started from their present impression of their school-days. This usually led on to work history. The conversation was openly recorded on tape. At a second session the other investigator administered the Wechsler-Bellevue Verbal Scale (including Vocabulary), and the Eysenck tests. For the control group the two sessions were reversed, in that we had to have the Vocabulary information first for the purposes of matching. If the Vocabulary score was not suitable, only a token test was added.

The Wechsler-Bellevue Scale. Vocabulary was used to match the groups. The rest of the verbal subtests were administered without diagnostic intention, but first in order to

provide a general indication of the relative status of the groups, and secondly to enable us to obtain a number of different sets of responses to items which, in respect of the degree of restriction placed upon the subject, would fall between the relatively narrowly controlled Moran tests, and the spontaneous utterances of conversation. The material is not analysed in detail in this paper, but certain points may be made as being of interest to those who regularly employ the Wechsler scales as a clinical instrument.

Over-all verbal I.Q.'s were computed. (The Vocabulary subscale could, of course, not be included in this.) Bearing in mind that the groups were equated in terms of Vocabulary score, from Table 1 we note that: the mean verbal I.Q. of the schizophrenic groups is approximately 90 against 96 for the normals, a difference that is statistically significant. On all five subtests the schizophrenics gave a lower mean performance, but the difference is significant only for Information and Similarities.

The Moran tests. Moran (1953) has described the details of the battery and its theoretical background elsewhere. The present study has used five subtests involving the manipulation of the same twenty-five well-known words: i.e. ten THING words (e.g. house, clock, clothes) and fifteen NON-THING words (e.g. friend, big, command). Then, for each word, the battery gives information on the subjects': (a) associations to the words; (b) breadth of understanding (number of synonyms available); (c) precision of understanding (proportion of synonyms correct); (d) ability to communicate with the words (sentence construction); (e) ability to form concepts with the words (Similarities test); (f) level of concepts formed; (g) ability to reason symbolically with the words (Analogies test).

Word association. This is the orthodox word association test, using the 25 key words as stimuli. It is not a test of ability, but an instrument for exploring the meaning context of each word. The hypothesis is that the schizophrenics' associations will show a less conventionally meaningful relation-

ship to the stimuli than will the normal subjects. The *a priori* scoring system designed by Moran was followed (as it was in all subtests), a four-point scale of relatedness being applied. From Table 1 it is seen that the mean schizophrenic score of 37.9 is significantly lower than the mean of 53.4 for the control group and the hypothesis is confirmed.

Synonym recall. Here, in effect, the subject is asked to give many definitions of the stimulus word. We are testing the limits of meaning. Cameron and others have said that the schizophrenic characteristically gives an approximate but related term in response to such a requirement. That is, their loose organization of concepts is demonstrated by imprecision. Hence the test is scored in two ways: (i) for the total number of correct synonyms, and (ii) for the proportion of correct synonyms to the total number given. It is seen that the schizophrenic group give significantly fewer correct synonyms, and more especially a lower proportion of correct ones. Note however, that only three-quarters of the responses of normal subjects are correct.

Sentence construction. Here we measure the ability to communicate more directly. It was speculated that even if understanding appears to be unimpaired, there might be difficulty in such connected verbal expression. The test consists of the 25 words again, the subject being asked to construct sentences incorporating one, two or three of them. The responses were scored 'blind' by a research student. The schizophrenic score is significantly lower. It is seen that the use of words in this type of communication is impaired.

Similarities. This is on the usual pattern, and is, of course, essentially a task in verbal concept formation. According to Goldstein the schizophrenic group should be less able to extract the necessary common features, and further if a similarity is found, then it will be at a lower level of abstraction than the control group's. Seven items here refer to THINGS, e.g. 'In what way are a door and a window alike?'; and ten to ABSTRACT concepts, e.g. 'In what way are these words alike: add, subtract, divide?' The responses are scored on two levels: (i) as Adequate, if the key words are not put into a category although some meaningful similarity is described, e.g. clock and ruler both have numbers; and (ii) as Abstract if the subject actually categorizes the words, e.g. clock and ruler are both measuring instruments.

The schizophrenics give fewer Adequate responses, fewer Abstract responses, and a lower proportion of Abstract/Adequate responses. However, in a comparison of concrete (THING) versus abstract (NON-THING) items, which is not shown in the table, the difference between the groups is not greater on the abstract than on the concrete items. Both the normal and the abnormal group find the abstract items harder, but not differentially so.

Analogies. The items are in the conventional form: 'Boat is to water, as car is to—float, land, wheels, ride.' To perform successfully here the subject must generate hypotheses about the relationships that might obtain between the first two words, but then discard all but the one relationship that 'fits' the third word with one of the alternative answers. The score is the number of correct completions and again the mean difference is highly significant.

The measure of availability. As has been indicated, in order to measure availability some sample of spontaneous language must be obtained. Although ideally we would have wanted a recording of unselfconscious conversation in the ward, this was obviously neither feasible nor ethically desirable. As the next best thing, a conversation with one of the authors was recorded with the permission of the subjects.

The variety of words available was then analysed by means of the Type Token Ratio (TTR) (Fairbanks, 1944). This is the number of *different* words used, per constant total. Consequently, from the transcribed conversation the first 1000 words spoken by the subject were analysed in this way. Tensegmental TTR's for 100 words each were computed, and a mean value obtained from these (as is usually done). The time taken to speak these one thousand words was also recorded.

To superficial inspection (i.e. by reading through them), the two sets of conversation showed no differences in respect of formal language characteristics. However, the TTR's are significantly different. (The absolute differences are small, but the TTR characteristically has a small variation with high stability.) The speech of the schizophrenics

is relatively dulled and lacks variety. Fewer words are used. In accordance with expectation, the time taken by the schizophrenic group is significantly longer.

DISCUSSION AND SUMMARY

The schizophrenic group shows uniformly inferior performance on the verbal tests. However, the differences between the groups appear to be in degree and not in kind. For the most part the groups show the same pattern of difficulty. The content of the schizophrenics' responses is not strikingly bizarre.

With regard to the abstract-concrete dichotomy, our data indicate again that this is not a simple question of presence or absence of a particular ability. When considering in detail each subtest dichotomized for its concrete and abstract *items* we have found that both groups find the abstract items more difficult. There are no significant differences between the groups in the degree of difficulty found. However, when the quality of the *responses* is considered, as in the Similarities test, the proportion of abstract responses is lower in the schizophrenic group. Not only do they give fewer abstract responses, after all they also give fewer adequate ones, but even in relation to this lower base-line, they give fewer. Hence within the limits of the test requirement, we must apparently conclude that although the schizophrenics can handle abstract ideas if necessary they are less inclined to deal with abstract concepts spontaneously.

This general language decrement is found in a group, all of whose members are accessible and apparently co-operative, and moreover where the ability to define words or the potential vocabulary has been equated at the outset, as had the obvious background variables.

The lack of strikingly bizarre responses and the pattern of small but consistent decrement in scores all the way from Information in the Wechsler-Bellevue to the Analogies in the Moran tests, appears to support the Chapman (1958) and Epstein (1953) findings of the exacerbation of normal errors in schizophrenics.

It leads us at this stage to doubt the idea of some fundamental specific disturbance in the thinking process due to schizophrenia *per se*.

We consider that the recent studies, with experimentally increased motivation, give further credence for this point of view. Cohen (1956) and Cavanaugh (1958) have shown that schizophrenic decrement on learning and concept formation tasks is not irreversible, but that normal performance can be induced if escape from aversive stimuli (shock and white noise through head-phones), is dependent on it.

Therefore, at the present, we would postu-

late something like a simple lowering or alteration in the attention process to account for our results.

ACKNOWLEDGEMENTS

This project was carried out with the financial assistance of the Medical Research Council, London. We are indebted to the Medical Superintendent and staff of the Purdysburn Mental Hospital, Belfast, for their generous co-operation. Thanks are likewise due to the medical superintendents and staff of the Forster Green and Whiteabbey hospitals.

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REVIEWS

The Doctor, His Patient and The Illness. By MICHAEL BALINT. (Pp. 355. 40s.) London: Pitman Medical. 1957.

With the steady application of natural science the face of medicine has acquired many a new look in the last fifty years. Such matters as the control, and even conquest, of killing infections and a surgical mastery, unheard of at the turn of the century, allow the argument that modern medicine is mankind's greatest achievement. It is platitudinous, however, that this achievement is grotesquely one-sided and that the readiness to extend and apply findings from physics and chemistry boldly to the art of healing is balanced by a carelessness about, even an antipathy for, the human sciences. Doctors who may have an exquisite knowledge of the behaviour of many organs and systems and some of their diseases yet have often to rely only on a psychology of mere 'common sense', with all the ignorance and bias that this may imply, for handling the subtle and unique distresses of their *patients*. In this country at least it is difficult to find a single medical school that offers any threat to man's sconces against recognition of his own unconscious. Modern medicine is largely a skilled mechanistic art paying a reluctant lip-service to organismal science. There may be many reasons for this but one at least is organizational.

The new medicine came from the University schools and could of course only make specialists and experts on human parts or systems. It can never make a speciality of the whole man as a living integrate, for such a speciality would require for its growth a set of circumstances never to be found in medical school or hospital; a familiarity with, rather than a mere knowledge of, minor illnesses and the psycho-social background on which they arise; a long-term view of, and acquaintance with, each patient; opportunity to acquire humility about the significance of positive organ findings and a curiosity about negative ones; a continuous study of people rather than of critical episodes; and the study of the significance of the doctor's behaviour in long-term relationships. It is only the general practitioner who has such a vantage-point and, by a paradox, the very

advances in natural science already mentioned are freeing him to use it. Whether he will do so is an open question, for his training and bias, like that of the other specialists, is only in the natural sciences. His new position promises much, if he can learn to use it.

Not so long ago the general practitioner was at the centre of medical and surgical drama, and it was he who would save lives by devoted and skilled work, and guide his patients through fearsome crises. Mortal illnesses were his domain, and minor or even major surgery was within his compass, so that he was truly the family's saviour from dire threat, and he and they could enjoy his heroic role and the prestige it brought. The growth of hospital facilities and specialists and advances in medicine drastically changed his role. He is no longer the family saviour, but rather the gateway to the hospital saviours, often a mere referral-agent for all sorts of conditions for which he once held the centre of the stage. Even if he saves lives now, it is undramatically and early, by antibiotics or by sending the patient to appropriate specialists. Maternity services, paediatrics, blood transfusion, anaesthesia, have much left his domain now, following even minor surgery into specialists' hands. The despairing attempts of his profession to help him occasionally taste the headier wines of medicine by seeking for him clinical assistantships in specialist hospitals have largely failed.

If his sick patients are either quickly cured by the new methods or sent to hospital away from his ken, what more is left for him?—minor ailments, vaccinations, sick notes, illnesses of old age, the exanthemata and neurotics? These are dull matters for one trained by specialists to worship exact specialist techniques derived from physics and chemistry, and it is small wonder that fears have grown among general practitioners, freed of the press and reward of treating major illnesses, that they are not so much the forward troops as the small fry of the profession. These fears will be justified so long as general practitioners see themselves only as humble pupils of venerated specialists, outposts who must remain orientated only to the natural sciences. If, however, they are prepared to undertake research into the phenomena they and only they singularly meet, general prac-

tice itself will acquire a newer and brighter look than ever. It could become the central speciality of medicine. It is to be noted, however, that this will not come until general practitioners can regard specialists as their assistants and not their masters. If medical schools and specialists cannot teach them this speciality, who will? There are several significant signs that general practice is undergoing a re-fit. The formation of the College of Practitioners was an important administrative step, a formal encouragement to its members to undertake research in their own subject, and to focus their gaze on other peaks than the spell-binding Royal Colleges with their splendid but lop-sided scientific achievements.

This book is another sign. It reports research into the processes of the doctor-patient relationship, undertaken over several years by fourteen general practitioners who met for regular discussions with the author, a psychiatrist. Dr Michael Balint's training seminars for general practitioners at the Tavistock Clinic are already famous, as is his method; for he does not teach but rather assists doctors to examine their own workaday problems critically, the usefulness or otherwise of their automatic personal responses, and the unconscious roles they take up when faced with patients; so that they can be freed to think afresh about problems under their noses. This book allows us to see why they have aroused such enthusiasm among scores of practitioners. The reader can follow the work and discoveries of one early seminar group, share in its arguments and examine its conclusions and concepts. With cheerful skill and modesty and with a singular lack of pretentiousness, the author describes the doubts, strains and bewilderments he and the group underwent during its research. The reader is also allowed to glimpse something of the personalities of these researchers—an essential for work which had to include the effect of these personalities on patients—and the account of the practices of the researchers thus has full-blooded earthy candour and rare authenticity.

From the first it is clear that the patients discussed are seen as living people, whose processes can be viewed dynamically and not on the natural-science model of static pre-Einsteinian physics. There is no account here of 'diseases', dehumanized syndromes, or 'behaviour'. The doctor, devoted and fallible, and his patient, suffering, puzzling, pathetic or irritating, challeng-

ing or ingratiating, are in the forefront of the studies presented; and the fateful consequences of each for the other are honestly offered the reader for scrutiny. This is medicine with the lid off. The study of hidden elements in object relations although newish is not novel; but there is a first report on the application of it to general practice.

It is not an easy work to summarize for it concerns a new point of view on many things. It illumines aspects of general practice hitherto only half glimpsed even by practitioners, and offers new unifying concepts to explain some of the well-known strains which have regularly and without profit beset both general practitioners and specialists in the past. It discusses some 'psychosomatic' illnesses of the sort for which respectable and traditional classification and generalization has been so sterile, merely by the way; whereas the uniqueness of the patients concerned and their transactions with their doctors may be fully examined. (The lesson for 'psychosomatic medicine' is a major one.) But several other words around which the fine medicine of natural sciences has somehow silently staked out private interests acquire in this book fresher and extended meanings. Even the word 'patient'—'that uninteresting vehicle of interesting maladies'—changes here almost to mean a fascinating vehicle of routine maladies. Certainly the words 'specialist' and 'general practitioner' take on a new look, and doctors as much as their treatments, are shown to be at the heart of medicine affecting decisively as if they were drugs, some of the very illnesses which the natural sciences have (within their limits) so much helped us to understand.

The seminar set out to develop general practitioner skills but quickly saw that the general practitioner deals not only with 'diseases' but with uneases of people and organ dysfunctions of protean form not yet organized into fixed patterns of organ disease ('unorganized' illnesses). Its members early learned, too, that the most important drug at this stage was the doctor himself, certain uses of which could cause both him and his patient unnecessary strain, suffering and irritation. The processes in the doctor-patient relationship which cause these were thereafter studied in a series of 'cases', some of which are reported at length in this book. Diagnostic signs of these processes came to be recognized and certain findings about therapy were made.

The conclusions of the seminar, here dogmatic,

there tentative, are reported in full but the reader can follow, too, how the author and the group grew the atmosphere necessary for frank reporting of success and failure, and how they granted to each other the freedom to criticize each other with usefulness and vigour. We can follow a doctor coming to recognize in such a setting his habitual, but not always useful, responses to his patients, and so understand some of the strains and rewards as the researchers came to recognize and modify their unconscious habits.

For general practitioners this book is likely to become a classic because of the findings reported, but it is certain also to have a *succès fou* among specialists, and teaching psychiatrists all over the world will welcome it. Yet it may have success of another kind. It could become a major change-agent of general practice.

The action-training and research here described could, if more widely practised, lead to a deepening and broadening of the scope of general practice itself. General practitioners often have a discontent with their limited skill in handling complex human problems. This book suggests that by adding to their training in natural science a preparedness to acquire skills in applied social science, their work can become more completely scientific, exciting and enjoyable.

Like Caesar's Gaul, the book is divided into three parts. The first explores matters of diagnosis of the whole patient as distinct from the specialist type of part-diagnosis. It demonstrates in early stages of distress in case after case that patients 'offer' various illnesses to their doctors and how the way an illness becomes 'organized' depends much on the practitioner's response to these early 'offers'. It reveals how the practitioners may be hindered from recognizing this matter for several reasons, among which is the belief that specialists should have the deciding word. It respects specialist opinion but shows how the practitioner often fails to fit the specialist's findings into a general picture, because of an over-respect for these as the final arbiter of diagnosis. Far from recognizing, treasuring and studying their own functions as integrators of medical findings, the researchers found that their relations to the specialist were *in statu pupillari*. They would abrogate their own central responsibility for final diagnosis and follow treatments prescribed for the part-diagnosis which is all any specialist can reasonably be expected to offer. Once their trained over-respect for super-

ficial physical diagnosis was realized, they became aware of the danger of resting content with physical signs, of surrendering ultimate responsibility to consultants and of 'diluting responsibility', so that eventually nobody need feel fully responsible for the outcome. This 'collusion of anonymity' emerged as a common feature of general practice, and with it the need for deeper forms of diagnosis by the general practitioner in the face of his patients' 'offers'.

The second part offers clinical evidence about the effects of giving advice and reassurance without full understanding of the real problem. It knocks sideways for the reader any content with 'commonsense psychology', but it comforts much by discussing alternatives. It tells 'how to start' to make fuller diagnosis and calling for a change in the doctor's characteristic modes of approach to problems (and hence of his personality), shows what happens when he listens with a synthesizing ear. The schools' alternative of routine questioning and rigorous medical history-taking is severely questioned. How to Stop, and the special advantages a general practitioner has for diagnosing and treating emotional disorders are discussed at length. Two cases thus handled by members of the seminar are fully reported. One is left astonished at how much now the doctors in the seminar could achieve in the treatment of difficult psychological problems.

The third section offers general conclusions; the apostolic needs of general practitioners to convert patients to his own standards and beliefs; and the unpredictable effects of this; how much regression should be permitted in the doctor-patient relationship; the effects of treatments causing privation to the patient; the doctor-patient relationship as a 'mutual investment company' which needs good managers and good researchers. But for medical psychologists the most interesting passages may be in a chapter which discusses the relations between psychological and bodily upset, between illness and disturbance of living, between 'conflict' and 'basic fault', the factors which influence the choice of 'organized' illness, and the dangers of compulsive psychotherapy.

An appendix is offered for the psychiatrist who may consider undertaking the training of general practitioners or medical students. It is of moment for all teachers of applied psychology. It describes how the goal of the acquisition of psychological skill was achieved and the danger of only intellec-

tual learning was avoided by the sentient handling of the group learning situation. The role of the group leader, the avoidance of dependent teacher-pupil relations, the countertransference of doctors to patients, each to the others and to the leader, are here discussed with skill and humility. In two other appendices the selection of doctors for this training and a follow-up of the cases earlier discussed are described.

The author has no wish to make obedient psychotherapists out of general practitioners. Rather he makes plain the uses (and here he goes far) and abuses (and here he is on solid ground) of psychotherapy in general practice. He shows how it must and can become an integral part of a doctor's daily work.

General practice emerges from this research as a rich field of work and further research, which may even contribute as much to medicine and psychology as it owes to these disciplines. This book is one of the first trails through this known but surprisingly ill-mapped territory, and must be saluted as a historic bit of pioneering.

T. F. MAIN

Family Influences and Psychosomatic Illness.

By E. M. GOLDBERG. (Pp. 308. 38s.)
London: Tavistock. 1959.

This book derives from an extended inquiry into the social and psychological background of duodenal ulcer made by the Social Medicine Research Unit of the Medical Research Council. Investigations were made into the background of thirty-two young men with proved duodenal ulcer by a team consisting of a psychiatrist, a psychologist and a psychiatric social worker. A well-matched series of control subjects was drawn from the list of a nearby general practitioner and was investigated in the same way. This book is written by the psychiatric social worker of the team and relates what she found in the social background of the patients and the controls. Her findings appear under the following chapter headings: the mothers' natal families and personalities, the mothers' attitude to the upbringing of their children, the fathers, the marriages of the parents, the childhood of the young men, and the functioning and cohesion of the families. There are plentiful clinical examples given and the discussion of the material is brief and cogent. There are also several appendices including a lengthy one by Mr V. B.

Kanter, the psychologist of the team, on his findings from psychological tests.

In studying family experiences in such a context as this, attention can be paid either to fairly precise and easily ascertained events, or to the underlying psychological or interpersonal systems inferred from them. The first approach allows simple theory and elegant arithmetic; the second complicated theory and either exceedingly complex mathematics or counting on the fingers of one hand. The first is methodologically beyond reproach and usually leads to negative results; the second is of doubtful reliability and often leads to interesting results. In so far as Miss Goldberg has some hypotheses relating to family relationships, she is able to tabulate her findings for and against. For example, she found that the mothers of young men with duodenal ulcer were more often over-protective, over-indulgent and over-restrictive than were mothers of control subjects. On such small numbers statistical tests of significance are rightly not even offered. The small numbers, on the other hand, carry a considerable advantage in that extensive quotation from the interviews was possible so that the reader may see what is meant by, for example, over-protectiveness, and what kind of evidence Miss Goldberg considers necessary for it. This goes some way to answering the question about reliability. Throughout the study there is an admirable use of quotation and detail that could only be derived from close association with the families and could only be presented provided the numbers were kept small. In general the reader gets no impression of being taken for a ride on a bandwagon as is so often the case in such collections of case histories. Although the findings are neither new nor overwhelming in their weight, they are substantial in their relevance to psychosomatic theory.

Mr Kanter's contribution is similarly painstaking and modest. Both projective techniques and questionnaires were used in the examination of the subjects and, while few of the original hypotheses were supported, several suggestive features came to light that beg for further elucidation. The authors of this book have attempted to describe the dynamic background of sufferers from duodenal ulcer without drastic over-simplification and without too much having to be taken on trust. This is a very difficult thing to do and within the modest limits they have set themselves, they have done it clearly.

R. H. GOSLING

The Rorschach and the Epileptic Personality.

By J. DELAY, P. PICHOT, T. LEMPERIERE and J. PERSE. (Pp. 265. \$6.) New York: Logos Press. 1958.

This book, first published in the original French in 1955, has already proved its value, and this admirable translation is greatly to be welcomed by those interested in either aspect of its subject. The methodology of the research project which the authors describe appears quite admirable, precisely planned and most faithfully carried out. They survey the literature of not less than eight countries on the epileptic personality, and discuss the published findings critically from various standpoints. The latter include those in which the epileptic personality is considered as innate or primary, on the basis of hereditary constitutional and genetic factors, and also those which consider personality patterns in epilepsy as acquired or secondary. Under this heading the epileptic personality is discussed as a reaction to the disease, as a manifestation of underlying lesions, as a result of medication, as indicating temporal lesions, and in relation to psychoanalytic theory, early and recent.

Literature relating to the use of the Rorschach with epileptic subjects is no less thoroughly and critically surveyed. Prof. Delay and his colleagues note that there is a wide variation in methodology in such studies, and so exclude, for purposes of their own experiment, all those which they rightly consider to be inadequately controlled or based upon insufficient data. Even after such elimination, the amount of appropriate material proved remarkably large.

The original work described and carried out by this French team involved fifty ambulatory epileptic patients in whom the diagnosis was confirmed both clinically and by electroencephalograph. The intellectual level of each patient was ascertained, and an attempt made to determine the aetiology and cerebral localization in each case. With refreshing self-critical candour the team notes that their population is not wholly satisfactory in that other factors of selection resulted in a relatively high proportion of temporal epilepsy. (One may wonder if this criticism is unfair in view of the recently increased facilities for diagnosing temporal lesions.)

To quote Pietrowski's foreword, 'this volume presents a model of what the Rorschach approach

to a diagnostic problem should be'. The results are both impressive and useful. The authors make a highly significant contribution to the vexed question of the epileptic personality by demonstrating this as bi-polar, on the basis of systematic study of the inter-relationship of variables. They distinguish two different types, subjects with a coartative personality being at the first pole, those with an extratensive personality at the second. 'The coartative subjects make a good social adaptation; the extratensive subjects make a poor social adaptation and show evident explosivity and impulsivity, sometimes of a serious character.' The importance of such differential diagnosis is obvious both for prognosis and as a means of assessing the degree of social adaptation possible for any epileptic subject. It is the more convincing because based upon a study carried out at the highest level of professional integrity.

THEODORA ALCOCK

Psychosomatic Methods in Painless Childbirth.

By L. CHERTOK. (Pp. 260. 35s.) London: Pergamon Press. 1959.

It is now many years ago that Grantley Dick Read first published his theories of 'Natural Childbirth'. Although there are some obstetricians who do not entirely agree with all of Dick Read's hypotheses, yet various aspects of his method are now in widespread use in this country and there can be little argument that these methods of preparation for labour have helped to bring considerable relief to many thousands of women. There are, however, still many centres where no preparation classes at all are given for women during pregnancy. In others, the training that mothers receive is not always of the standard that is really required.

In this book, Dr Chertok makes an extremely thorough review of the various measures of preparation that have been evolved, including the Read Method and also the more recent Psychoprophylactic Method, which has been practised in the Soviet Union and more recently in France and many other European centres. In some ways the Read Method and Psychoprophylaxis have certain features in common, but in others they are rather different. The latter method requires highly skilled teachers, who have a real insight into the problems of pregnancy and labour, but the results

seem to justify this extra expense of time and manpower.

Naturally, this book does consider various aspects of the psychology of pain, but since this in itself is such a vast subject, it does not consider all the aspects involved.

This book will make absorbing reading for all those who are interested in helping women to adapt themselves satisfactorily to the experience of labour. It is certainly the best book that I have read to date on this subject.

NORMAN MORRIS

Recovery from Schizophrenia. By JOHN EISELE DAVIS. (Pp. 184. 36s.) Springfield: C. Thomas; Oxford: Blackwell. 1957.

Relaxation and Motivation methods are described for treating the chronic, mute and regressed catatonic schizophrenic, who has failed to respond to all other therapies. It is based on the work of Paul Roland, a sensitive and gifted physical therapist, who has used a combination of techniques through which the patient is brought to successive stages of reality contact. At the start he massages the patient's back and face, talking softly in an encouraging way, and aiming at physical and mental relaxation. A friendly relationship is built up with a single therapist under the doctor's guidance, and the patient is led away from autism into activity through successful performance. The activities are individually tailored and may follow a pattern of normal childhood development, learning to talk and play with later more complex learning and socialization over 18 months. The patients live in a special unit with one therapist to four to five patients. There is a warm and cohesive group atmosphere, with high dedication and tolerance of failure on the part of the staff.

This work must be seen against the background of the old back ward, where patients were allowed to regress completely. Regressed schizophrenics have been shown to improve on a variety of techniques utilizing greater staff participation and interest. For example, controls in drug trials show significant improvement in behaviour. Controlled trials evaluating these techniques show gains in behaviour and socialization, with evidence of schizophrenia still present, although some patients were able to leave the hospital. It is with

the provision of specific physical experiences that any new approach is signalled.

Body communications are provided in a continuous way. During massage, one hand of the therapist never leaves the patient's body. To assist talking, tongue depressors and lip moulding are used. Generally speaking, the variables are too manifold to be rated accurately. The author acknowledges the presence of transference phenomena, but then tries to explain the elements of change in behavioural and James-Lange terms. These seem inadequate and finally meaningless.

Here lies a fruitful field for research into the early and regressed ego as a body ego. Controlled physical manoeuvres could perhaps be assessed for their value only when unconscious phantasies are explored in later intensive therapy.

ISMOND ROSEN

Textbook of Psychiatric Nursing. By A. P. NOYES, E. M. HAYDON and M. VAN SICKEL. (Pp. 415. 33s.) London: Macmillan. 1958.

The fifth edition of this textbook offers a brief account of the whole field of psychiatry and nursing. The latest development lies in the accentuation of the therapeutic nurse-patient relationship as part of a dynamic team approach. To illuminate this, elements of Freudian theory are propounded so that they may become familiar.

The nurse is shown how to mind her emotional p's and q's with the patient, and to provide a social ward environment, optimum for cure. But some fundamental problems which ought themselves to be examined in psychoanalytical terms remain obscure. The first of these is the meaning and degree of 'meeting the emotional needs of the patient'. This has become almost a catch phrase covering infinite ranges of care, from buffering hostility, providing warmth, understanding and acceptance, to developing social skills. The directing psychiatrist will of course determine these, and the book tacitly leaves it to him. Next, by what method the nurse will attain the ideal therapeutic attitudes is left in abeyance. Unless the nurse can act in an ego-syntonic way, all the 'do's and don't's' remain futile.

While this *rapprochement* between formal psychiatric nursing and psychodynamics is welcome and simply written, it remains incomplete. There is as yet no systematic analytic theory of nursing,

the approach, as exemplified here, being largely empirical.

The book may be of value for the nurse in the middle of her training, covering, as it does, the area in a general way.

ISMOND ROSEN

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Psychological Stress. By IRVING L. JANIS.
(Pp. 439. 56s.) New York: John Wiley;
London: Chapman and Hall. 1958.

Based on psychoanalytic and behavioural studies of surgical patients, this book formulates and tests hypotheses pertinent to a general theory concerning human emotional reactions and thought-processes in extreme situations of external threat and danger. The author includes new theoretical concepts and research findings on major problems concerning: normal adjustment mechanisms, unconscious conflicts and modes of resolution, motivations underlying increased need for group affiliation, and conditions under which authoritative communications are accepted or rejected. A detailed case description is given, and the major psychoanalytic hypotheses emerging from this study of one patient are assessed in the light of additional evidence presented in the second half of the book. This additional evidence is derived from behavioural studies using two different approaches—systematic intensive interviews of a representative sample of surgical patients, and correlational data from a questionnaire survey administered to several hundred young men who had recently undergone surgery.

Instinctive Behavior. Translated and edited by
CLAIRE H. SCHILLER. (Pp. 328. 45s.)
London: Methuen. 1959.

The papers contained in this well-illustrated volume are classics in the development of the science of Ethology, and are among the most original and important contributions to the analysis of instinctive behaviour. For the first time available in English, they appear here in a readable translation. Part 1 consists of Jakob von Uexküll's pioneering study, 'a stroll through the worlds of animals and men'. It is chiefly concerned with demonstrating how the environment

appears to a great variety of animals in terms of their inherent sensory capacities. Part 2 contains papers by Lorenz, Tinbergen and the late Paul Schiller. These authors attempt to unravel the intricate network of environmental influences which activate and modify innate behaviour mechanisms.

Guide to Using the Mill Hill Vocabulary Scale with the Progressive Matrices Scales.
(Revised edition.) By J. C. RAVEN.
(Pp. 64. 5s. 6d.) London: H. K. Lewis.
1958.

Guide to Using the Coloured Progressive Matrices, Sets A, Ab, B. (Revised edition.)
By J. C. RAVEN. (Pp. 40. 5s.) London:
H. K. Lewis. 1958.

These guides are now available from the publishers as separate publications, as well as being included in specimen sets of the tests to which they refer. They each give a brief historical introduction, and a careful account of the theoretical principles on which each test is constructed.

The guide to the Mill Hill Vocabulary Scale contains, for the first time, a section on the standardization and experimental work carried out with this test in conjunction with the Matrices Test, with particular reference to its use in clinical work, and for the comparative study of verbal disfunction in mental illness. Perhaps most important of all, both guides now contain norms from the age of four onwards and for normally healthy old people up to eighty-five years of age. An item analysis of each scale, and tables of intercorrelations at various ages, together with a full qualitative study of the data obtained, are given. These are for use in evaluating the test responses under normal and abnormal mental conditions. Standard test instructions etc., given in previous editions, and a bibliography of publications to date are included.

The guides now review the whole range of intellectual development in health and illness, changes in acquired information and ability for verbal communication, from infancy to extreme old age. The new guides are chiefly concerned, however, with the uses of the matrices in vocabulary tests with young people, old people, and in clinical work.

Selected Writings of John Hughlings Jackson. Vols. I and II. Edited by JAMES TAYLOR. (Pp. 500 and 510. £6. 6s. set.) London: Staples Press. 1958.

With the publication of this two-volume set, totalling over 1000 pages, John Hughlings Jackson's principal writings, which thoroughly revolutionized the foundations and practice of modern neuropsychiatry, are again available after being long out of print. The papers show the development of Hughlings Jackson's thought—from his early investigations of convulsive seizures, through the researches into the nature of epilepsy, chorea and hemiplegia, up until his mature years, when his interests were no longer mainly diagnostic, but had come more and more to embrace the philosophical. Volume II concludes with the only complete listing available of all of Jackson's extant works.

Personality Patterns of Psychiatrists. Vols. I and II. By ROBERT R. HOLT and LESTER LUBORSKY. Menninger Monograph, no. 13. (Pp. 386 and 400. \$7.50 and \$4.00.) New York: Basic Books. 1958.

Volume I attempts to answer the question of what personality characteristics are most important in a good psychiatrist. The question is asked—how can a training institution scientifically evaluate a prospective psychiatrist's ability to master the lengthy, demanding preparatory work? The authors attempt to provide satisfactory answers to these questions. Their findings are based on their ten-year research study of 466 applicants for psychiatric training at the Menninger Foundation, and this volume attempts to formulate systematic methods for testing, rating and selection within an original theoretical framework. Illustrative material drawn from the case of one resident is analysed in detail.

Volume II is intended to be read in conjunction with volume I, and contains matter introduced or briefly treated in the earlier volume. Details of the statistical and rating techniques are given, as well as an account of problems and techniques in judging proficiency. There is a section on how to use letters of recommendation, application forms and other credentials validly. The appendices present expert opinion on needed qualities of

personality for psychotherapy and psychoanalysis.

Speech and the Development of Mental Processes in the Child. By A. R. LURIA and F. IA. YUDOVICH. (Pp. 126. 15s.) London: Staples Press. 1959.

This Russian study reports a systematic investigation of the development of speech in a pair of identical twins, and also throws light on the part played by heredity and environment in the growth of mental life. This is the first report of this kind to come from the Soviet Union and has been translated by Joan Simon, with an introduction by Prof. Oliver Zangwill.

Ego Psychology and the Problem of Adaptation. By HEINZ HARTMANN. (Pp. 121. 21s.) London: Imago. 1959.

This is the first full English translation (by David Rapaport) of a classic essay which represented a turning point in the development of a psychoanalytic ego psychology. Hartmann attempts to stake out the field of ego psychology in its broadest sense. He introduces here the concepts of the undifferentiated phase, conflict-free ego development and conflict-free ego sphere, and primary and secondary autonomy. He discusses the role of endowment and inborn ego apparatuses, and the fact that there is a certain pre-adaptedness to the environment even in the human being. Hence, defences may simultaneously serve the control of instinctual drives as well as adaptation to the external world. Hartmann also discusses the implications of his theories for the development of perception and thought processes, and for the concepts of ego strength and ego weakness, and the concept of normality. The monograph was first published in German in 1939.

Conceptual and Methodological Problems in Psychoanalysis. Vol. 76, Art. 4. Annals of the New York Academy of Sciences. By LEOPOLD BELLAK, M. OSTOW, E. PUMPIAN-MINDLIN, A. H. STANTON and T. S. SZASZ. (Pp. 262.) New York: The Academy. 1959.

This monograph focuses interest on conceptual problems in psychoanalysis. A large section constitutes an attempt at the systematic restatement of

the libido theory (by Szasz, Stanton and Pumpian-Mindlin). There is a section on the unconscious by Bellak and on the structural model by Ostow. The lively and critical discussion is given in full.

The Nature of Stress Disorder. (Pp. 298. 25s.) London: Hutchinson Medical. 1959.

This book consists of the fifteen papers read before the Society for Psychosomatic Research in May 1958, together with verbatim reports of the discussions which followed them. It is fully documented and illustrated.

The papers fall into five groups. The first deals with essential hypertension, pruritic skin conditions and dyspepsia. The second, under the general title 'experimental stress' opens with a paper on stress in aviation and includes papers on other responses to stress situations in men and animals. The third group considers stress disorder in society, stress in industry and the university student. The fourth group, dealing with the person in his family setting, includes papers on the family doctor and stress in childhood. The final group of papers is concerned with the influence of genetic endowment, and includes a paper on the genetics of peptic ulcer and hypertension.

The Psychoanalytic Study of the Child. Vol. 12. (Pp. 417. \$8.50.) New York: International Universities Press. 1957.

The contributions to this volume include a number of theoretical papers. Charles Brenner writes on the nature and development of the concept of repression in Freud's writings, Phyllis Greenacre on the childhood of the artist, Edith Jacobson on normal and pathological moods, Jeanne Lampl-de-Groot on defence and development, Rudolph Loewenstein on interpretation in the theory and practice of psychoanalysis, and Seymour Lustman on psychic energy and mechanisms of defence. The section on aspects of early development contains papers by Gabriel Casuso, Marianne Kris, William G. Niederland, and a joint paper by three workers at the Hampstead Clinic on 'inconsistency in the mother as a factor in character development'. In addition there are a number of clinical contributions and two papers on applied psychoanalysis.

The Psychoanalytic Study of the Child. Vol. 13. (Pp. 573. \$8.50.) New York: International Universities Press. 1958.

In this volume the Ernst Kris memorial meeting papers are given together with the comments of eminent discussants. Of particular interest is a paper by Anna Freud on child observation and prediction of development. In the theoretical section Heinz Hartmann contributes a paper on the scientific aspects of psychoanalysis, and there is one by Max Schur on the ego and the id in anxiety. The chapters on normal and pathological development are divided into three—adolescence, latency and preoedipal phase, with contributions from K. R. Eissler, Anna Freud, Elisabeth Geleerd, Leo Spiegel, Annie Reich, David Beres, P. J. van der Leeuw, and René Spitz. There are also a number of papers on clinical contributions and applied psychoanalysis. A complete list of the writings of Ernst Kris are included in this volume.

The Annual Survey of Psychoanalysis. Vol. 5. Edited by JOHN FROSCHE and NATHANIEL ROSS. (Pp. 608. \$12.00.) New York: International Universities Press. 1959.

Volume 5 of this, by now well-known, survey, presents the psychoanalytic literature in a set of comprehensive sections. These deal with history, critique, theoretical studies, clinical studies, dream studies, psychoanalytic child psychiatry, psychoanalytic therapy, psychoanalytic training, applied psychoanalysis and psychoanalytic books. This last volume deals with the literature for 1954.

Aion. Volume 9, Part II of the Collected Works. By C. G. JUNG. (Pp. 344. 32s.) London: Routledge and Kegan Paul. 1959.

The central theme of this volume is the symbolical representation of the psychic totality through the concept of the self, whose traditional historical equivalent is the figure of Christ. Prof. Jung demonstrates his thesis by an investigation of the *Allegoria Christi*, especially the fish symbol. Gnostic and alchemical symbolism are considered, and are treated as phenomena of cultural assimilation. The astrological aspect of the fish symbol and, in particular, the foretelling of the Antichrist are discussed in detail.

Complex Archetype Symbol. By JOLANDE JACOBI. (Pp. 236. 21s.) London: Routledge and Kegan Paul. 1959.

This study, translated from the German, deals with three of the fundamental concepts in the psychological theories of C. G. Jung. Dr Jacobi investigates the interrelationships of these concepts and their meaning and importance, and compares the role of the symbol in the theories of Jung and in the psychoanalytical theories of Freud. In a final section, the author analyses a child's dream and finds in it examples of the archetype and the symbol.

The Fifth Mental Measurements Yearbook. Edited by OSCAR KRISEN BUROS. (Pp. 1292. \$22.50.) Highland Park: Gryphon Press. 1959.

Once again this monumental work presents an exhaustive review of the test literature in the seven-year period—1952 to 1958. It attempts to list all the commercially available tests—educational, psychological and vocational—published in the English-speaking countries, as well as all measurement books. It is intended to be used in conjunction with the previous Yearbooks.

A Search for Man's Sanity. The Selected Letters of Trigant Burrow. (Pp. 615. 60s.) New York: Oxford University Press. 1958.

Burrow, who early in his career diverged from the main stream of psychoanalysis, questioned 'normality' as a criterion for health and became increasingly aware of the social implications of neurosis. In the twenties he turned from his practical work and embarked on an experiment in which he initiated an intensive group analysis together with his students and associates, and in which the motivations of each member were relentlessly scrutinized. Burrows saw evidence of a biological cause for conflict and proposed that, through the appropriate physiological technique, the source of human strife could be defined and controlled. His views were expressed in a prodigious correspondence with many eminent persons, many of whom are represented in these selected letters.

M.K.P. Myokinetic Psychodagnosis. By EMILIO MIRA Y LOPEZ. (Pp. 186. \$6.75.) New York: Logos Press. 1958.

Prof. Mira describes his method of myokinetic diagnosis as a means of studying expressive behaviour, and this is the first accessible account of the method in English. Prof. Mira lists the dimensions of temperament which he believes are tapped by his method. Among these are depressed and euphoric tendencies, intrapsychic coherence (integratedness), excitation and inhibition, anxiety and apathy, aggressiveness, introversion and extroversion, and certain pathological trends. He likewise believes that a test of intelligence may be derived from his battery, even though it is non-verbal.

Psychology of the Child. By ROBERT I. WATSON. (Pp. 662. 56s.) New York: John Wiley; London: Chapman and Hall. 1959.

This book approaches child psychology as an integral part of the subject matter of general psychology. It includes material from educational, clinical, social and general psychology, and critically examines the contributions of each of these to the field of child psychology. Learning theory is integrated with psychoanalysis. The author presents his material in developmental sequence from infancy through early childhood to later childhood. The school-age child is seen in an educational setting and an attempt is made to relate normal to abnormal development.

Group Processes. Edited by BERTRAM SCHAFFNER. (Pp. 266. \$4.50.) New York: Josiah Macy, Jr. Foundation. 1959.

The transactions of the Fourth Conference on Group Processes include discussions of the impact of game ingredients on children's play behaviour and the implications for our current models of personality. Group size, interaction and structural environment receive consideration as well as the role of aggression in group formation.

A Psychological Study of Typography. By Sir CYRIL BURT. (Pp. 68. 15s.) Cambridge University Press. 1959.

This is an experimental and statistical survey of the mind's capacity to take in a printed message. Tests have been applied to the problem of the

legibility of various styles of printing and certain standards are recommended. Sir Cyril Burt has made use of factor analysis in assessing the various characteristics of typeface and their relation to personal reactions.

Mental Illness in London. By VERA NORRIS. Maudsley Monographs, 6. (Pp. 317. 35s.) London: Chapman and Hall. 1959.

This extensive inquiry is based on the analysis of clinical records, social background and subsequent history of nearly 10,000 patients admitted to mental hospitals and observation units in the London area between 1947 and 1949. The effect of sex, age and marital status on the incidence of mental disorder has been evaluated. The prospects of recovery in the various forms of such illness are described, and these have been found to be rather less reassuring in certain respects than has been widely assumed. The epidemiological and economic issues involved have been appraised, and the endemic rate of mental illness has been found to exceed the epidemic rate of most infectious diseases, and the burden imposed on the economic resources of the community is considered.

The Language of Psychology. By GEORGE MANDLER and WILLIAM KESSEN. (Pp. 301. 54s.) New York: John Wiley; London: Chapman and Hall. 1959.

This book offers an analysis of scientific language in psychology, tracing its development from the everyday vernacular to the formulation of explicit theories. It deals with such contemporary issues in philosophy and psychology as phenomenology, operationism, definition, induction, the nature of explanation, and the creation and structure of scientific theories. The authors emphasize recent developments in the logic and philosophy of science, and discuss the use of psychological findings and theory in an understanding of traditional philosophical problems.

Readings in Psychoanalytic Psychology. Edited by MORTON LEVITT. (Pp. 413. \$8.50.) New York: Appleton-Century-Crofts. 1959.

Twenty-four distinguished authors contribute to this collection of readings which deals with general considerations in ego psychology, developmental

psychology, the ego and defensive processes, psychoanalytic diagnosis, theory and technique, and applied psychoanalysis. This volume of source material contains a number of classical papers, and collateral reading lists have been appended to the articles by the editor.

Brain: Memory Learning. By W. RITCHIE RUSSELL. (Pp. 140. 18s.) London: Oxford University Press. 1959.

Recent important additions to our knowledge of the operations of the nerve-cells of the central nervous system have led to Dr Ritchie Russell's reconsideration of mental processes, in so far as they depend on the activity of brain-cells. The fourteen chapters include an extensive consideration of memory and the traumatic amnesias. Word mechanisms, consciousness, pain, attention and inhibition as well as learning are discussed in detail.

Sherrington: Physiologist, Philosopher and Poet. (No. IV of the Sherrington Lectures.) By LORD COHEN OF BIRKENHEAD. (Pp. 108. 17s. 6d.) Liverpool: University Press. 1958.

This set of three Sherrington lectures includes a comprehensive account of Sherrington's professional life, and an appreciation of his work as a physiologist and as a philosopher and poet. It includes a complete bibliography of Sherrington's writings.

The Excitable Cortex in Conscious Man. (No. V of the Sherrington Lectures.) By WILDER PENFIELD. (Pp. 42. 10s. 6d.) Liverpool: University Press. 1958.

Following an introductory appreciation of Sherrington's work, Dr Penfield discusses the material and the hypothesis of central organization, and then describes the various aspects of cortical stimulation. He deals with the visual, auditory and somatic sensory areas of the cortex. The motor cortex and motor responses are described and discussed. A large section of these lectures is devoted to a consideration of psychical responses, with reference to experimental hallucinations and interpretive illusions, and the notion of the interpretive cortex is put forward.

On the Mysterious Leap from the Mind to the Body. Edited by FELIX DEUTSCH. (Pp. 273. \$5.00.) New York: International Universities Press. 1959.

This book is an outcome of Dr Deutsch's workshop discussions on psychosomatic problems, conducted in collaboration with the Boston Psychoanalytic Society. He is concerned with the fundamental problem of the mind-body interrelationship, and makes a specific contribution to the theory of conversion both as a process and a symptom. The papers and discussions included scrutinize the formative stages of conversion; its relation to sensory experiences, symbol formation, identification, and the aggressive drives; its manifestations in psychosis, homosexuality, and disturbances of the menstrual cycle, and its role in determining choice of symptom.

Operational Values in Psychotherapy. By DONALD D. GLAD. (Pp. 326. 68s.) London: Oxford University Press. 1959.

This work presents an account of the principal techniques and processes of change in dynamic psychotherapy. From an examination of the actual operations of psychotherapists and group leaders, a framework is developed within which clinical flexibility in interviewing, scientific objectivity in the study of interpersonal processes, and conceptual economy in understanding theories of psychotherapy may be achieved.

The author presents a simplified account of the dynamic psychotherapies as a basis for more efficient practice and research in the treatment of interpersonal problems, emotional difficulties, and mental illness. He also attempts to provide technical methods for learning, applying and investigating psychotherapy and group leadership skills.

Dynamics of Behavior. By ROBERT S. WOODWORTH. (Pp. 403. 36s.) London: Methuen. 1959.

This new book by Prof. Woodworth deals with the dynamic interaction of motivation, perception and learning, and is to some extent supplementary to his *Experimental Psychology*. He describes his basic orientation, and the various chapters deal with behavioural dynamics at the molar level, motives in operation, outgoing motivation,

theories of motivation, the control of muscular movement, perceiving the spatial environment, the perception of objects, learning the environment (sequence learning, cue learning, discrimination learning, places and things), concept learning and problem solving.

Christ and Freud. By ARTHUR GUIRDHAM. (Pp. 193. 21s.) London: Allen and Unwin. 1959.

The primary concern of this work is with the question of psychiatric factors in religion, and religious factors in psychiatry. Dr Guirdham believes that orthodox Christianity is a perversion of the psychologically irrefutable teaching of Christ, and that its conception of God as a supreme being, its teaching on the resurrection and its contamination with a sense of guilt, are especially conducive to psychiatric disorder. He also deals with the psychological origins of clericalism and attempts to show the role it plays in stifling religious experience. The spiritual significance of sex is discussed as well as the prospect of closer collaboration between priests and doctors.

Television and the Child. By HILDE T. HIMMELWEIT, A. N. OPPENHEIM and PAMELA VINCE. (Pp. 592. 42s.) London: Oxford University Press. 1958.

The terms of reference of the inquiry reported in this work were to study the impact of television on children and young people. It is based on a series of studies carried out by social psychologists over four years, and draws on the observations of parents and teachers, and on the detailed examination of children. In one area the changes brought about by television could be directly traced by examining children both before and after television had come to the area. The research indicates how misleading are generalizations about the effects of television, and how greatly these effects vary with the child's age, sex, intelligence, home background and personality.

Behavioral Analysis. By DAVID M. LEVY. (Pp. 370. 72s.) Springfield: C. Thomas; Oxford: Blackwell. 1958.

Dr Levy's careful studies represent an attempt to raise our level of thinking about human behaviour. He has developed an organized method of analysis-

ing clinical data, and has relied on a careful study of individual variations and the conditions which bring them about. Included are studies of sucking behaviour, the infant's resistance to sucking, mother's behaviour when experiencing pain and frustration, her methods of stimulating the baby to suck and of displaying affection and annoyance. Detailed description of his methods and observations are given.

Self-Destruction. By BEULAH C. BOSSELMAN. (Pp. 94. 36s.) Springfield: C. Thomas; Oxford: Blackwell. 1958.

This book is concerned not so much with the problem of suicide *per se* as with the problem of human self-destructiveness in general. The author believes that an act of suicide, representing the most extreme form of negation of life, can contribute to insight into the nature of circumstances, personal and environmental, which create anger and despair. A thorough review of the literature is given, and statistical conclusions concerning the external circumstances associated with suicide are derived. Intensive psychiatric case studies are quoted.

The Dispossessed. By LAURA LONGMORE. (Pp. 334. 30s.) London: Jonathan Cape: 1959.

Johannesburg is by far the most heavily industrialized city in Africa and it is there that the problems of the dispossessed, detribalized Africans are greatest. They live in overcrowded urban surroundings and are kept in racial, social and economic subjection. Miss Longmore has investigated, and describes in great detail, what life is like in the African areas of Johannesburg, especially for women and girls. Her findings have led her to analyse the effect both upon human nature and African thinking of the prevailing uneasiness and uncertainty, and her general conclusion is that if the social maladjustment and unrest she has uncovered are allowed to increase they may lead to disaster.

Verbal Behavior. By B. F. SKINNER. (Pp. 478. 42s.) London: Methuen. 1959.

This book represents an attempt by Prof. Skinner to account for 'verbal activity', i.e. speech, reading, listening, in terms of behaviour theory. He

explains verbal behaviour without recourse to the customary semantic terms of meaning, significance and reference.

The materials are not drawn from laboratory experiments, but poetry, selections from novels, and everyday speech are analysed, using principles derived from laboratory experiments on learning. The general problems of how language is learned, retained and produced are tackled.

Differential Treatment and Prognosis in Schizophrenia. By ROBERT D. WIRT and WERNER SIMON. (Pp. 198. 50s.) Springfield: Charles Thomas; Oxford: Blackwell. 1959.

This work reports a research project into schizophrenia which includes multidisciplinary evaluations by psychiatric examination, psychometrics, and social work techniques. The patients selected for this project were first admissions, had no history of previous treatment for schizophrenia and had no prior tranquillizing medication. Evaluations were made of demographic material, social history factors, behaviour rating schedules, a personality test, intelligence estimates, psychiatric rating scales and an occupational adjustment scale. The authors reach conclusions concerning the effectiveness of chlorpromazine and reserpine.

The Care of the Geriatric Patient. Edited by E. V. COWDRY. (Pp. 438. 60s.) St Louis: C. V. Mosby Company; London: Henry Kimpton. 1958.

The nineteen chapters in this volume are all written by experts in the geriatric field. The relation of the physician to the geriatric patient is discussed as well as psychological and medical aspects of geriatric care. Problems of surgery, anaesthesia, the administration of drugs and nutritional requirements are dealt with as well as nursing, hospitalization, rehabilitation, and the care of the geriatric patient in his own home. Problems of training in geriatrics are also discussed.

Suicide in Hong Kong. By P. M. YAP. (Pp. 101. 12s. 6d.) Hong Kong: University Press; London: Oxford University Press. 1959.

Dr Yap has analysed both statistical and personal data relating to suicide and attempted suicide. He

interprets the new pattern of suicide incidents as reflecting in part the new hardships endured by the aged.

The Central Nervous System and Behavior. Edited by MARY A. B. BRAZIER. (Pp. 450. \$5.25.) New York: Josiah Macy, Jr. Foundation. 1959.

The First Conference on The Central Nervous System and Behaviour (sponsored by the National Science Foundation and the Josiah Macy, Jr. Foundation) reported here, deals in the main with Russian contributions to the understanding of behaviour, and the background of the Russian neurophysiologists is described and discussed. The work of Danilevsky, Wedensky, and Ukhomsky as well as that of Pavlov and Bechterev is described, and further sections deal with summation, brain stimulation and conditional reflexes, electroencephalographic studies and correlates of conditioned learning.

The Adolescent through Fiction. By NORMAN KIELL. (Pp. 345. \$5.00.) New York: International Universities Press. 1959.

Dr Kiell attempts to set down the dynamic principles of adolescent psychology and development, and he illustrates these concepts by excerpts from contemporary fiction. He highlights the richness of the adolescent's ubiquitous and absorbing experiences.

Involutional Melancholia. By ÅKE STENSTEDT. (Pp. 71. No price.) Copenhagen: Ejnar Munksgaard. 1959.

This is an aetiological, clinical and social study of endogenous depression in later life, with special reference to genetic factors.

An Experiment in Mental Patient Rehabilitation. By HENRY J. MEYER and EDGAR F. BORGATTA. (Pp. 114. \$2.50.) New York: Russell Sage Foundation. 1959.

This report describes an evaluation of a programme for the rehabilitation of post-hospitalized mental patients. The experience of conducting the research is described, with a record of problems encountered. The analysis of the data collected, including a factor analysis, is thought to be relevant to knowledge about mental patients, their

backgrounds, and the factors associated with their remaining in the community or being re-hospitalized.

Psychiatry in Medical Practice. Edited by W. LINDESAY NEUSTATTER. (Pp. 311. £2. 2s.) London: Staples Press. 1959.

This is a survey of the ways in which psychiatry can contribute to medical practice in general. It is divided into two parts: a straightforward description of the more common psychiatric disorders and a detailed outline of special techniques and treatments in psychiatric practice.

Community Mental Health. By MARGARET C.-L. GILDEA. (Pp. 169. 37s. 6d.) Springfield: Charles Thomas; Oxford: Blackwell. 1959.

The author presents a classification for distinguishing severely sick children from those with mild disturbance. She defines levels of emotional adjustment in elementary school-children and attempts to show how distinction can be made from simple observation in the normal social setting, school and home, rather than in the clinic. Case data are given.

The Nature of the Self. By FRANCIS J. MOTT. (Pp. 316. 42s.) London: Allan Wingate. 1959.

The author puts forward the theory that all creation occurs according to a single and unvarying pattern, and he attempts to trace in the mind the same pattern found in physical forms.

Dreaming. By NORMAN MALCOLM. (Pp. 128. 12s. 6d.) London: Routledge and Kegan Paul. 1959.

The author examines the common view that dreams are mental activities or mental occurrences taking place during sleep. He starts off by offering a proof that the sentence 'I am asleep' is a senseless form of words and cannot express a judgement. After commenting on various features of the concept of sleep, the author expands his argument to prove that the notion of making any judgement at all while asleep is without sense. The discussion is extended to other mental acts and occurrences.

The Quest for Identity. By ALLEN WHEELIS. (Pp. 250. 21s.) London: Victor Gollancz. 1959.

Dr Wheelis draws on his experience as a psychiatrist and discusses the question of the loss of our identity and our search for a new one. He discusses the influence of changing patterns of value imposed by society, and the dilemma for the individual which arises out of this. The difficulties which now arise in the building of a durable identity and the anxieties which arise from this are described.

Manual of Psychiatry. By K. R. STALLWORTHY. (Pp. 365. 30s.) Christchurch, N.Z.: N. M. Peryer. 1959.

This work, from New Zealand, is now in its fourth edition. It is intended as an elementary text in psychiatry.

Theory and Methods of Scaling. By WARREN S. TORGERSON. (Pp. 460. 76s.) New York: John Wiley; London: Chapman and Hall. 1958.

This book is designed to provide a convenient single source for studying the various theories and methods of measuring psychological attributes. Prepared at the request of the Committee on Scaling Theory and Methods of the Social Science Research Council in the United States, it contains material previously available only in isolated articles or in special chapters of books devoted primarily to the subject matter of a particular field. New and more complex methods such as the multidimensional scaling models, latent structure analysis, and the general categorical judgement model are included.

The Mentally Retarded in Society. By STANLEY POWELL DAVIES. (Pp. 248. 45s.) Columbia: University Press; London: Oxford University Press. 1959.

The author describes the historical background from which present methods of caring for the mentally deficient were developed. He presents information about modern practices and indicates present and future needs for an adequate programme. This book deals with such problems as

social indictment, sterilization, segregation, defective delinquents and changing concepts of causation. He discusses the modern institution, colonies, family care and community programmes, vocational training and employment and the socializing process. Dr Davies's conclusions are based on data presented in the text.

The Sixth Sense. By ROSALIND HEYWOOD. (Pp. 223. 21s.) London: Chatto and Windus. 1959.

This is an account of extra-sensory perception, and represents a critical summary of the evidence for psi phenomena as collected by scientific techniques. Mrs Haywood describes the experiments initiated by Henry Sidgwick and Frederic Myers, and the results later obtained by Gilbert Murray in the field of telepathy. There are accounts of mediumship subjected to stringent test conditions and of the experiments in extra-sensory perception made by J. B. Rhine, G. N. Tyrrell, S.G. Soal and others. The book ends with a survey of the implications of psi for religion, philosophy and psychology.

The Healing Voice. By A. PHILIP MAGONET. (Pp. 205. 18s.) London: Heinemann. 1959.

This represents an attempt to explain hypnosis in simple terms, with special reference to its value in the relief of complaints of nervous origin brought about by the stress of contemporary life. Particular attention has been paid to psychosomatic symptoms, and some case histories are quoted.

Readings in Social Psychology. Edited by ELEANOR E. MACCOBY, THEODORE M. NEWCOMB and EUGENE L. HARTLEY. (Pp. 674. 55s.) London: Methuen. 1959.

This is an up to date and comprehensive collection of papers illustrating most of the ways in which the influence of social conditions upon psychological processes have been studied. The editors have attempted to provide selections that are complete in themselves rather than truncated extracts from books. The emphasis is on the 'research report' rather than on theoretical discussion of the nature of the field of social psychology. Many of the papers have been written

specially for this book, and there are substantial changes in the content matter of this latest edition.

The Demonstration Clinic. By DAVID M. LEVY. (Pp. 120. 37s. 6d.) Springfield: Charles C. Thomas; Oxford: Blackwell. 1959.

This work includes an account of certain new techniques in the approach to the joint evaluation of mother and child. The technique of the 'demonstration clinic' is described in detail.

Psychology, the Nurse and the Patient. By DORIS M. ODLUM. (Pp. 200. 15s.) London: Iliffe. 1959.

The attitude towards nursing has in recent years undergone a change in the direction of an increased psychological orientation, and the importance of the role of the nurse is in the process of being reassessed. Dr Odum's book, now in its third edition, covers the whole of the psychological part of the syllabus for both the preliminary and final examinations for State Registered Nurses. Two chapters have been added to cover the new Mental Health Act.

The Szondi Test. By LIPOT SZONDI, ULRICH MOSER and MARVIN W. WEBB. (Pp. 309. 96s.) London: Pitman Medical. 1959.

This book discusses the foundation, rationale, validation, tenets, and diagnostic and clinical applications of the Szondi test and its theoretical background of *Schicksalsanalyse*.

Szondian depth psychology is described in detail, and chapters are devoted to shock therapy, group testing, and forensic psychology.

An Outline of Human Relationships. By EUSTACE CHESSER. (Pp. 446. 25s.) London: Heinemann. 1959.

Dr Chesser traces the growth of personality from infancy to maturity and suggests what should be achieved at each stage of development if the individual is to reach his full status as a human being. There are chapters on industry and on war and peace as well as on the need for religion.

Hypnosis—Fact and Fiction. By F. L. MARCUSE. (Pp. 224. 3s. 6d.) London: Penguin Books. 1959.

This work attempts to offer a broad view of the field of hypnosis. General problems of hypnosis as well as its application to the clinical, dental and medical fields are considered. The author's main purpose is to separate fact from fiction in this highly controversial subject.

Mental Retardation. By HANS MAUTNER. (Pp. 280. 35s.) London: Pergamon Press. 1959.

This collection of lectures by Dr Mautner emphasizes the physiological base of the many conditions which lead to mental retardation and especially the relation of brain function to autonomic regulations. Clinical descriptions as well as an extensive bibliography are included.

Released Mental Patients on Tranquillizing Drugs and the Public Health Nurse. By IDA GELBER. (Pp. 139. \$3.00.) New York: University Press. 1959.

The work reported in this book includes a survey of the problem, and the needs of patients as identified from the records of released mental patients and as reported by state mental hospital authorities are considered. There are chapters on a proposed public health nursing follow-up programme and the implications of this study for further research.

Evolution of Nervous Control from Primitive Organisms to Man. Edited by ALLAN D. BASS. (Pp. 231. 52s.) London: Bailey Bros. and Swinfen. 1959.

This book is the outcome of a symposium organized by the American Association for the Advancement of Science. It portrays the impact of the evolutionary process upon the brain. The various papers represent a variety of disciplines and view-points which are presented by a botanist, an embryologist, a neurologist, a neurophysiologist, a physiologist, a psychologist and a psychiatrist. The work concludes with a chapter on psychoanalysis and human behaviour by I. Arthur Mirsky.

Insulin Treatment in Psychiatry. Edited by MAX RINKEL and HAROLD E. HIMWICH. (Pp. 386. \$5.00.) New York: Philosophical Library. 1959.

The current status of insulin therapy in psychiatry is evaluated, and the many contributors to this work present a number of controversial viewpoints.

The Onset of Stuttering. By WENDELL JOHNSON and associates. (Pp. 243. 40s.) Minnesota: University Press; London: Oxford University Press. 1959.

On the basis of a study of parents of 500 children, half of whom were stutterers and half non-stutterers, conclusions relating to the onset of stuttering are reached. The Minnesota Multiphasic Personality Inventory was extensively used in this research, and the numerical results are given in detail.

Biochemistry of the Central Nervous System. Edited by F. BRÜCKE. (Pp. 324. 88s.) London: Pergamon Press. 1959.

This volume contains the latest information on the central nervous system by leading experts. It is volume 3 of the edited proceedings of the Fourth International Congress of Biochemistry, Vienna, now being published in fifteen volumes for the International Union of Biochemistry.

Neuropharmacology. (Transactions of the Fourth Conference.) Edited by HAROLD A. ABRAMSON. (Pp. 285. \$5.) New York: Josiah Macy, Jr. Foundation. 1959.

The contents of this volume include studies of the effect of respiratory poisons and anoxia on Siamese fighting fish in relation to LSD-25 reaction, clinical studies with tarazein, 'stop' and 'start' systems, and relations between chemical structure and physiological action of mescaline and related compounds.

Time Distortion in Hypnosis (second edition). By LINN F. COOPER and MILTON H. ERICKSON. (Pp. 206. 32s.) London: Baillière Tindall and Cox. 1959.

Time distortion is defined as a great variance between the actual or clock time and the seeming duration of the time during which some event occurred. Dr Cooper has found that such a

phenomenon can be deliberately induced under hypnosis, and he sets forth the methods and results of his experimental studies in the first part of the book. In part 2, the clinical phenomena involved in time distortion with specific reference to therapeutic applications are discussed and studied by Dr Erickson. Dr and Mrs Erickson collaborate on the third section 'subjective time condensation as distinct from time expansion'.

The Social Adjustment of Children. (Manual to the Bristol Social Adjustment Guides.) By D. H. STOTT. (Pp. 46. 25s.) London: University Press. 1959.

The method described by Dr Stott, directed at the assessment of the personality and temperament of children, provides a standard means of assessment of behaviour from the point of view of stability or maladjustment. His method has been to select, by repeated statistical validation, a large number of behaviour indications such as can be reported upon unambiguously by teachers, staff of residential centres and social workers. Descriptions of behaviour which proved vague or insufficiently revealing were discarded or modified until they proved satisfactory. In this way a reasonably objective means of assessing a child's social adjustment has been evolved.

The diagnosis is based upon a comprehensive view of the child's behaviour and, in the case of maladjustment, upon a number of symptoms. Since only those symptoms have been retained which were very characteristic of disturbance, a combination of several such symptoms makes the possibility of error small.

Basic Issues in Psychiatry. By PAUL V. LEMKAU. (Pp. 106. 25s.) Springfield: Thomas; Oxford: Blackwell. 1959.

The five chapters of this report of a series of lectures deal with the size and range of mental health problems, the preservation of central nervous system tissue, the prevention of psychogenic illness, treatment in psychiatry, and the administration of psychiatric programmes.

Readings in General Psychology. Edited by PAUL HALMOS and ALAN ILIFFE. (Pp. 251. 25s.) London: Routledge. 1959.

The editors have selected fifteen papers in psychology by a number of well known experts. Their

intention is to provide a text-book which adopts an intensive and detailed view of a limited number of topics rather than one which is panoramic and superficial.

Four Picture Test. By D. J. VAN LENNEP. (Pp. 15. \$10.00; £3. 12s.) Utrecht: Nederlandse Stichting voor Psychotechniek. 1958.

This test is a projection test, designed in 1930 by Dr van Lennep, director of the Netherlands Institute of Industrial Psychology at Utrecht. The test consists of four coloured pictures, which represent different general situations. The subject is made to construct one story to include and combine all four pictures. The test can only be given in its written form and it is applied both as an individual and as a group test to subjects from the age of 10 years onwards. The stories produced appear to be very personal and contain projections of characteristics and conflicts, specially those reflecting the general attitude towards life of the subject.

Exploration in Group Relations. By E. L. TRIST and C. SOFER. (Pp. 68. 7s. 6d.) Leicester: University Press. 1959.

This is a report of a residential conference held in 1957 by the University of Leicester and the Tavistock Institute of Human Relations. The antecedents of the work undertaken are discussed in an introductory chapter, and this is followed by chapters on the design and organization of the project, the experience of the conference, a follow-up report, and the next steps. The appendices include a section on the study group method of training.

Dimensions of Transference in Psychotherapy. By BERNARD APFELBAUM. (Pp. 90. \$1.75.) Berkeley: University of California Press. 1958.

A report of the use of *Q*-technique in the investigation of transference in psychotherapy.

Social Psychiatry and Community Attitudes. (World Health Organization Technical Report Series, no. 177.) (Pp. 40. 1s. 9d.) Geneva: World Health Organization. 1959.

This is the seventh report of the Expert Committee on Mental Health. It considers the nature and

goals of social psychiatry, the community and its mentally ill, community attitudes and psychiatric practice, recommendations for action and for research.

Mental Health Problems of Ageing and the Aged. (World Health Organization Technical Report Series, no. 171.) (Pp. 51. 3s. 6d.) Geneva: World Health Organization. 1959.

This report, the sixth of the Expert Committee on Mental Health, considers the extent and sources of the problems, the protection and promotion of mental health in old age, the classification and prognosis of mental disease in old age, the organization of geriatric mental health services, specific treatment measures and rehabilitation, and training of personnel and organization of research. It concludes with a number of recommendations.

Ataractic and Hallucinogenic Drugs in Psychiatry. (World Health Organization Technical Report Series, no. 152.) (Pp. 72. 3s. 6d.) Geneva: World Health Organization. 1958.

This report of a study group contains an evaluation of our present lack of knowledge. Psychophysiological correlates of some psychotropic drug effects and their relation to a theory of the psychoses, problems facing a provisional classification, and the relation of drug-induced mental changes to psychoanalytic theory are considered. Attention is given to problems posed by the use of these drugs in the treatment of psychoses, the differences in the effects of, and needs for, psychotropic drugs in different cultures, sources of error and the need for methodological tools, and some public health aspects of the use of psychotropic drugs. The first appendix lists representative members of the principal groups of psychotropic agents, while the second contains a list of supporting documents.

Soviet Medicine in the Fight against Mental Diseases. By L. ROKHLIN. (Pp. 165. 5s.) London: Lawrence and Wishart. 1959.

A popular account of the history and present practice of psychiatry in the Soviet Union.

Psychopharmacology Frontiers. Edited by NATHAN S. KLINE. (Pp. 533. 72s.) London: J. and A. Churchill Ltd. 1959.

These papers represent the proceedings of the Psychopharmacology Symposium of the Second International Congress of Psychiatry. There are 65 papers dealing with general problems of psychopharmacology, clinical observations, specific problems, and mode of action. Three sessions of a discussion of pharmacological treatment of schizophrenics are reported in detail.

Psychotherapeutic Drugs. By ASHTON L. WELSH. (Pp. 139. 36s.) Springfield: C. Thomas; Oxford: Blackwell. 1958.

The author presents this monograph in the hope that a description of the various psychotherapeutic agents, their indications for use, and their side effects, will enable physicians everywhere to be guided in their treatment and alerted to manifestations of reaction in their patients.

PROCEEDINGS OF THE MEDICAL SECTION OF THE BRITISH PSYCHOLOGICAL SOCIETY

296. 22 October 1958. Dr MARIE JAHODA: Current Concepts of Mental Health.
297. 26 November 1958. Dr J. BOWLBY: Studies of Parent-Infant Interaction in Man and Animals: some Notes on Projects in Progress.
298. 17 December 1958. Dr S. I. ABRAHAMS: The Integrating Role of the General Practitioner.
299. 28 January 1959. Dr A. PLAUT: Aspects of Consciousness (Address from the Chair).
300. 25 February 1959. Dr I. C. KAUFMAN: Some Psychological Factors in the Aetiology of Epilepsy.
301. 25 March 1959. Mr M. REED: Communication and the Deaf.
302. 27 May 1959. Dr H. REY: Studies on the Relationship of Hormones, Brain Activity and Behaviour in Women.
303. 24 June 1959. Dr L. Z. FREEDMAN: Research in Psychiatry and Law.





